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BRITISH MOSSES.

By the Rt. Hon. Lord Justice Fry, F.R.S., F.S.A., F.L.S.

(*Continued from page* 43.)

BUT, it will be said, assuming that this may be the case with one growth of forest, how about the successive destruction of successive forests? The answer is, I believe, to be found in the curious change which peat undergoes, and which converts it from a substance highly absorbent of water into one impervious to it.

The section exposed by a peat-cutting in, I believe, almost all cases exhibits two kinds of peat, the one known variously as red peat—or red bog, or fibrous bog, or in Somersetshire as white turf—which lies at the top, and the other a black peat, which lies at the bottom. The red peat retains visible traces of the Sphagnum of which it is mainly composed, and is highly absorbent of moisture; whilst the black peat has lost all, or nearly all, traces of the minute structure of the cells, and is not only unabsorbent of moisture, but is impervious to it. In fact, it constitutes an insoluble substance which is said to be scarcely subject to decay, so that it is used in Holland for the foundations of houses, and is found unchanged after ages, and when the buildings have fallen into decay. It is even said to have remained unchanged after three months' boiling in a steam-engine boiler. The broad difference between these two kinds of peat may easily be ascertained by anyone who will, as I have done, subject the two kinds to the action of water.

In Fig. 33 will be found a section of a peat bog, copied from an engraving in the third Report of the Commissioners on Irish Bogs ("Parliamentary Papers," 1813-4), and exhibiting the remains of three forests anterior to the vegetation growing on the surface of the bog. The history of the formation will be, I believe, much as follows:—

(1) We must get a watertight bottom. In the section given it is said to consist of limestone gravel, but this probably had, at least in its lower part, got consolidated into a pan by the infiltration of insoluble iron oxides, themselves often due to decaying vegetable matter, or it rested on a subsoil of stiff clay. The necessity of this watertight bottom is well shown by the fact that in places in the Irish bogs where a pure limestone subsoil occurs the bog becomes shallow and dry.

(2) On this limestone gravel a forest arose and flourished for a considerable period, until the natural drainage of the area was stopped, whether by the choking up of the course of the effluent stream, or from the aggregation of vegetable matter, or from the fall in the course of nature of the trunks of the trees themselves. Everyone who will consider how much care our rivers require in order to make them flow with regularity to the sea—who thinks, for instance, of the works in the Thames valley, or in the upper valleys of the Rhine—will see how often and how easily, in a country in the condition of nature, stagnant waters will arise. In the morass thus formed the Sphagnum has grown, years after years, and if it has not destroyed the old trees it has prevented the growth of young ones. The stools of the trees buried in the antiseptic waters of the Sphagnum pools have been preserved, whilst the fallen trunks have, except when preserved by the like circumstance, rotted, and added their remains to the peat which

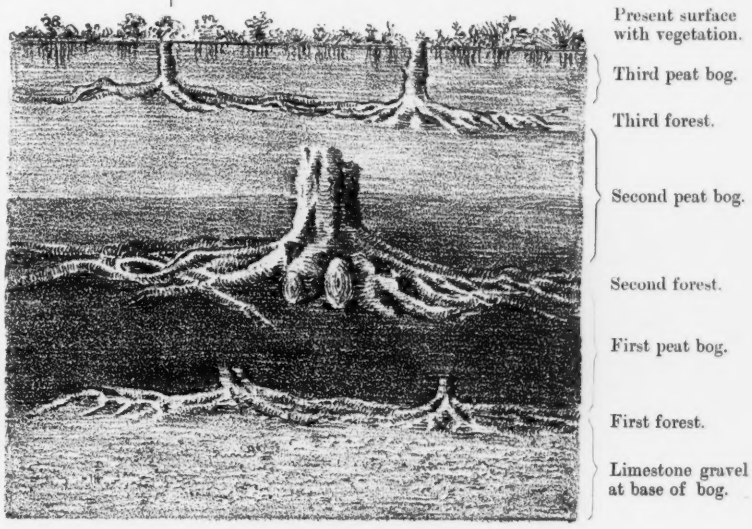


Fig. 33.—Section of Irish Peat bog, showing the growths of three successive submerged forests. (*From Third Report of Commissioners on Irish Peat Bogs.*)

the Sphagnum has been producing. It has been observed in several places in Scotland, that the under side of fallen trees which would be protected from decay by the tannin of the Sphagnum is preserved, whilst the upper side has decayed or rotted away. Year by year the process of decay on the lower parts of the Sphagnum went on until the water grew shallower and at last disappeared, leaving the original morass choked and filled up by the Sphagnum and the plants which it has nourished. On the top of this soil have grown first the heath and bog shrubs which first succeed the Sphagnum, and in time, as the soil has grown more solid, forest trees. This is our second forest. This first peat deposit, or the lower part of it at all events, having been turned into the black peat impervious to water, plays the same part in the second stage that the clay or pan did in the first stage. Again, the drainage of this second level got stopped, and the forest bottom loaded with stagnant water, the home of the Sphagnum; together, the water and the Sphagnum killed the forest trees, which share the fate of their predecessors. The same history is gone through again—the Sphagnum filling up the morass and turning the water into dry land until it supports the third forest.

Decay of the Moss.—There comes, however, in many cases a time when this process is arrested; the artificial drainage of the soil, or the physical position of the area, prevents the re-formation of a morass, and the Sphagnum dies away. So in many parts, if not universally, in Sedgmoor, in Somerset, it is almost impossible to gather a bit of Sphagnum, and the peat is well known to the turf diggers not to be reproduced. Here the regulated drainage of the level maintains the surface in the condition of meadows or agricultural land. But in many cases, especially on mountain sides or tops, when the Sphagnum has died, and the peat undergone its last change into black earth, a process of decay sets in under the influence of air and water. The water lies in holes or "hags," or flows in sluggish streams, wearing away the dead peat; and the surface of the soil is broken and uneven, small patches of green surface with a rough growth of sedge or grass being surrounded by wider spaces of black earth. Such is, or was some years ago, the condition of the peat on the top of Kinder Scout in Derbyshire; on the parts of Dartmoor around Cranmere Port; and such also it is described to be on many of the Lowland hills of Scotland.

Sedgmoor.—In some cases the Peat Mosses have been originally arms of the sea, and the peat has only grown after the exclusion of the salt water. Such appears to be the history of Sedgmoor, the great plain of Central Somerset. Northward it is bounded by the Mendips; eastward lies Glastonbury with its Tor or hill; westward the Bristol Channel. The plain is intersected by the low line of the Pouldon Hills, once a long level-backed island or promontory in the estuary and afterwards in the morass; and the way in which the villages lie and the moor is apportioned between them suggests that the Pouldon Hills and some other spots which slightly rise above the level of the Moss were the original seats of population. Originally this whole area appears to have been open to the Bristol Channel, of which it formed a bay or recess. The Burtle beds are a marine deposit well seen at the slight elevation on which the village of Burtle stands, which have been traced in various places along the borders of the moor and indicate the old line of beach. A curious confirmation of this geological fact is afforded by the presence—the one on Shapwick Heath, and the other near Glastonbury—of two plants (the *Rumex maritimus* and the *Vicia lutea*) which are shore plants, but which have until recently maintained their places as remains of the ancient marine flora, showing the retreat of the sea. The *Vicia lutea* has, I believe, recently succumbed in this interesting locality to the British collector. The description of Glastonbury as the Isle of Avalon, and the account of the bringing of the body of King Arthur from Tintagel to its resting-place at Glastonbury, are confirmations from tradition of the same fact.

Then a change came over the district, apparently by the formation of barriers of sand or mud along what is now the shore of the Bristol Channel, and along the sides of the overflowing rivers, and in that way the sea-water was shut out, and a depressed region left with a mud surface; on this there arose a forest of oak, ash, and yew, then the water stagnated, and in it the Sphagnum grew, and gradually filled it up, killing the growth of trees on the surface of the land, but leaving, down to historic times, spaces of fresh water from which the Abbots of Glastonbury formed their great fishing lake at Meare, by the side of which they erected the beautiful manor house and fish house which still remain. When the Romans occupied this part of England, they not only used the Burtle beds for plastic clay, but used the peat in their kilns, and the remains of the road which they constructed across the moor are now found some six feet below the present surface. In like manner, a pathway exists across part of the moor near Westhay consisting of slabs of birch, and perhaps alder laid crosswise, so as to form a kind of corduroy road. This has been found in one place at a depth of seven, at another of two feet only beneath the surface. The road bears the name of the Abbot's path, or way, and it may well, I think, have been a way by which the monks of Glastonbury passed from their abbey by way of Meare to Burtle, where they appear to have had a chapel which they served. Now, as I have already said, the system of drainage is so complete that the peat, when once cut, is not reproduced (though the lower soil is said to have a remarkable power of expansion and rises often to the old level), and the Sphagnum is to be found rarely, if at all, on many parts of the moor.

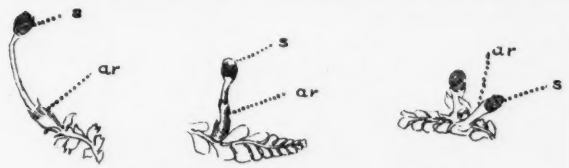
To the intimate structure of the Turf Moss are thus to be attributed great results in the history of the world. To look at our own island alone, but for it the primeval forests that once covered the land might still be standing; but for it large tracts of land would still be lake and mere; but for it every freshet in a Highland river would be a flood; without it we should have had no Mosses on the confines of England and Scotland, and where would have been the border warfare and the border minstrelsy? where the Moss hags in which the hunted Covenanters sought for shelter and freedom of worship? To come southward, by force of its growth, the broad meadows of Somerset have been built up, and the dark waters on which the mysterious barge bore the dead Arthur from Tintagel to Avalon have been turned into the green pastures of Glastonbury and Meare and the battle-field of Sedgmoor.

Hepaticeæ. If my reader will once again refer to table A, he will see that the Hepaticeæ, the lowest group of Mosses, when that word is used in its wider signification, yet remain for some little notice. I am afraid that most people slight them greatly, and feel little inclination to examine them; and yet they possess a beauty of their own—a great diversity of form, and points of great interest and importance to the botanist.

Popularly these little plants would probably be considered Mosses, and it may be hard to say whether or no they deserve the name, and so botanists use two Latin words for the one English one, and allow them to be Muscineæ, though not Musci.

In the springtime, anyone who carefully looks may find,

in woods or on shaded banks, little plants, in such forms as those shown in Figs. 34, 35, and 36. These show two



Figs. 34, 35, 36.—*Jungermanniæ*. *ar*, archegones; *s*, sporangium. After nature.—A.F.

species of Jungermannia—of the family Jungermanniaceæ of my table A. The genus bears a somewhat uncouth name, and one wishes it had a pleasanter one than this, which was bestowed upon it in honour of a worthy German botanist. It has been said that the names of the Mexican kings before the conquest rob them of the fame which their merits deserve; and so long as these little plants bear this name, I see no hope that they should ever attain popularity.

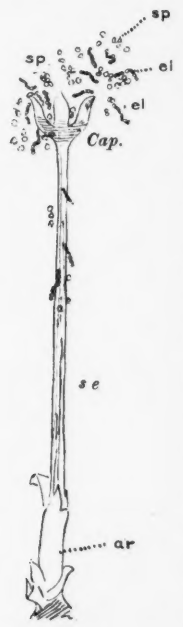


Fig. 37.—Part of a *Jungermannia*, magnified; *ar*, archegone; *se*, seta or stalk; *cap*, capsule of four leaves opening and emitting *sp*, spores and *el*, elaters. After nature.—A.F.

That these little plants have great likeness to ordinary Mosses must be conceded, but the student who looks at them attentively will soon see something different about their general appearance. This is partly due to the peculiar way in which the leaves are inserted—the lines of attachment of the leaves to the stem generally forming an angle like a V. But other differences will be apparent from the figures. The sporangium, as in the Mosses, takes its rise from an archegone. In the Mosses this organ is concealed in modified leaves, and is generally speaking not easy to detect. In these Jungermanniaceæ the archegones form conspicuous green organs (*ar*) in which, in the earlier stages, the dark-coloured sporangia appear to rest (Fig. 36, *s*). They are subsequently raised on a seta or stalk, generally of a perfectly white cellular structure. The sporangium is not divided into capsule and lid, but opens as shown in Fig. 37, by the springing backward of the four leaves which constitute it, and emits large quantities of spores (*sp*), interspersed with very curious structures, known as elaters (*el*). These elaters, with some attached spores, are seen more highly magnified in Fig. 38. The elaters are endless threads, twisted into spirals, and when liberated the spirals rapidly twist and twirl about, and in so doing, aid in dispersing the spores. The spectacle of an opening sporangium of a Jungermannia is a very interesting one.

It is remarkable that in the true Mosses, with their much more highly organized capsules and spore cases, these elaters have disappeared.

If the patience of my reader hold out, I will ask him, for the last time, to refer back to my table A, where he will find that last of all in the series come the Marchantiacæ, so named from the best known species of the group, the *Marchantia polymorpha*, a plant so remarkable and so worthy of full consideration, that I fear that if I embarked on it I should weary my reader beyond endurance, so I leave it, at least for the present.

Fig. 38.—Spores and elaters of *Jungermannia* magnified. *sp*, spores; *el*, elaters. After nature.—A.F.

Distribution in Time.—It will be interesting now to enquire how long the present Moss flora of England has existed. How far back can we carry our knowledge of the existence on the world's surface of these delicate organisms? It is evident that they have had but a small chance of leaving evidence of their existence as fossil remains, because whilst the strong, almost wiry, vessels of the ferns have a great power of resisting decay and so getting preserved, the delicate cellular structure of the Mosses offers little or no resistance to that process. Hence it is that the fossil remains of Mosses are not very numerous, or for the most part very ancient. Yet we have some materials to answer the enquiry. Three ancient collections of Mosses enable us to throw some light upon it. In an interglacial bed near Crofthead, in Renfrewshire, eleven species of Moss were discovered, and with one possible exception all are well-defined British species of the present day. If we take Mr. Wallace's chronology, and hold that 80,000 years have passed since the Glacial epoch disappeared, and 200,000 years since the Glacial epoch was at its maximum, we may perhaps give from 100,000 to 150,000 years for the age of this little collection. Out of the eleven Mosses discovered, seven belong to the genus Hypnum, or the family Hypnaceæ. This collection, then, is evidence, so far as it goes, (1) that the existing Moss flora is as old as the interglacial epoch; (2) that the Hypnaceæ were as dominant then as now; and (3) that the specific forms have remained constant since that epoch.

Another collection of fourteen Mosses has been discovered in a drift in the Clyde valley above the Boulder drift, and tends to confirm the previous conclusions; as all the species are existing, all now inhabit the valley of the Clyde, and the Hypnaceæ are still predominant, though not in so great a proportion as in the Renfrewshire bed.

A third collection has been found at Hoxne, in Suffolk, in a lacustrine deposit, probably resting in a hollow in the boulder clay. Together with phanerogams of an arctic habit, have been found the remains of ten Mosses, which are described by Mr. Mitten as looking "like a lot of bits drifted down a mountain stream." They are all still dwellers in our island, and exhibit, like the other collections, a preponderance of the family of Hypnaceæ.

But we can give some evidence of more ancient date. Heer inferred the existence of the Mosses in the Liassic period from the presence of remains of a group of small Coleoptera, the existing members of which now live amongst Mosses—an inference which seems not very strong. But recently the remains of a Moss have been found in the carboniferous strata at Commentry, in France. It appears to be closely allied to the extant Polytrichum, the most highly-developed genus of Mosses; so that we have here a phenomenon like that which occurs in reference to the Equisetacæ and Lycopodiaceæ, viz., that the earliest fossil species known belong to very highly-developed forms of the group.

Distribution in Space.—If we turn, now, from the distribution of this family in time to its distribution in space, we shall observe some curious phenomena. Of our English Mosses some are of almost world-wide distribution; some are found here and in spots far removed from our shores; some are believed to be peculiar to our island. One observation should be borne in mind in considering the following statement, viz., that Mosses are often small and inconspicuous plants; that they are often neglected where flowering plants have been collected and studied; and that consequently the statements as to their non-existence must always be accepted with this proviso, that they may mean the non-existence or the non-discovery, or non-observation of the plant in question.

Of our Mosses I have said that some are cosmopolitan in their extension. Our common *Funaria hygrometrica*, our *Bartramia pomiformis*, and, amongst the Hepaticæ, our Marchantia are a few amongst many that are denizens alike of the Old and of the New World. Of the New Zealand Mosses about one-fifth are British, or at least European, species; of the Tasmanian Mosses about one-third.

But whilst there are instances of this wide dispersion, there are instances too of the opposite kind. Amongst the British Mosses and Hepaticæ (as we learn from Mr. Wallace on the authority of Mr. Mitten) seventeen Mosses and nine Hepaticæ are said to be peculiar to the British Islands, and of these, three genera of Mosses and three of Hepaticæ are also non-European (*i.e.*, not known on the Continent of Europe). The three non-European genera of Mosses have their greatest development in the Andes; the three non-European genera of Hepaticæ have their greatest development in the temperate regions of the southern hemisphere. Let me take one of the Mosses of which the genus is non-European—Streptopogon. This genus is thus distributed: Seven species in the Andes; one in the Himalayas; three in the south temperate zone; one in Sussex. Take, again, one of the Hepaticæ, *Acrobolbus*; its species are confined to New Zealand and the adjoining islands, with one species in Ireland.

There are other noteworthy but isolated facts about the distribution of Mosses to which I may here refer: one is, that the great boulders of the plains of the centre of Germany are found to bear alpine species of Moss as if brought from some distant or elevated region; is this due to the actual transport of the boulders on which they live or to the retreat of the northern flora in the tail of the ice as it retreated northward as the glacial period disappeared? In any event it reminds one of the scattered patches of alpine gentians, which are to be found in the great stretch of moorland south of the Danube, and between that river and the Tyrolese Alps.

I have reason, furthermore, to believe that if the Mosses of some parts of the south-west of England were worked out carefully, northern forms would be found to prevail in a way which would require, if possible, some explanation.

The facility with which the wind can carry the small spores of the Mosses probably accounts in some cases for the wide distribution of the organisms. The very small area occupied by some species suggests great susceptibility to local surroundings.

Conclusion.—I can cordially recommend the study of the Mosses to any, old or young, who really love Nature: I have found in it a great source of pleasure during the last few years. The tops of walls, the banks of lanes, the slopes of woods, the mountain passes, each inhabited by different classes of Mosses, are as distinct in their vegetation as the oak or elm or beech counties of England, or the pine-clad slopes or the birch groves of the Alps. A square foot, in some situations, will contain a large number of species of different forms and modes of growth. The long arms of the Hypnum may stretch along the ground, whilst the Tortulas raise their spires of rich brown from out rosettes of verdant leaves, and the Bryums with their pendant capsules vie with them in beauty. One stone or bit of boggy land may be a study in colours—greens, browns, reds, greys, and gold—which my pen would fail to describe. A wall-top may show

> "A stubble field, or a canebrake; a marsh
> Of bulrush whitening in the sun."

Another may present a mimic forest, built up of varied forms, as different from one another as were the huge vegetables of the Coal period from our trees. In a word, I find myself, whenever in the country, surrounded by a world of beauty and interest which I only dimly perceived before I entered on the study, though I have never, I hope, been entirely unobservant of things around me. More than ever I can say—

> "In small proportions we just beauties see,
> And in short measures life may perfect be."

"But how shall I begin the study?" some may say. Gather the first moss you come across, examine it with the naked eye, and then with a microscope, and you will have made some advance. If the British Museum be accessible to you, go to the Botanical Department and examine the collection beautifully arranged and exposed in one of the rooms upstairs. But books—you must have books to aid you, and therefore I will suggest a few. Bagnall's "Handbook of Mosses" will, I believe, be found a very useful first book, and is very inexpensive. "The Handbook of Cryptogamic Botany," by Bennett and Murray, will be a very good one with which to begin the study of the organization of the Mosses. Berkeley's "Handbook of British Mosses" may serve as the second book on classification. Wilson's "Bryologia Britannica" is a more advanced book of the same description, and difficult to get. Dr. Braithwaite's "British Moss Flora," which is in course of publication, is a more elaborately illustrated and expensive book. Two works of Schimper's—"Recherches sur les Mousses" and "Entwickelungsgeschichte der Torfmoose"—are admirable, and his "Synopsis Muscorum Europæorum" is very helpful, especially for finding European species not found in Britain. The work of old Dillenius ("Historia Muscorum"), though of course out of date, is very delightful, and contains many excellent plates, worthy of study even in the present day.

One pleasant duty remains to be performed. Figures 4, 5, 6, 9, 10, 11, 15, 17, 21, 22, 23, 24 have been copied from Schimper's "Recherches sur les Mousses"; Figures 16, 27, 28, 29, 30, 31, 32, from the same author's "Entwickelungsgeschichte der Torfmoose"; Figures 7, 8, from Berkeley's "Handbook." For permission to use these sets of figures I beg to thank respectively Messrs. Freutel and Weitz, Mr. Schweizerbart, and Messrs. Lovell Reeve and Co.; nor can I omit to add my thanks to my daughter, Agnes Fry, from whose drawings, partly original and partly copies, most of the foregoing illustrations have been taken.

ELEPHANTS, RECENT AND EXTINCT.

By R. Lydekker, B.A. Cantab.

(*Continued from page* 47.)

We turn now to the grinding or molar teeth, which present far more remarkable peculiarities, and of which a brief account was given in the article on "Teeth and their Variations," already published in Knowledge. So peculiar, indeed, and unique are the structure and mode of succession

of the molar teeth of Elephants, that they are often very imperfectly understood, even by those who have spent half their lives among these animals. For instance, we find

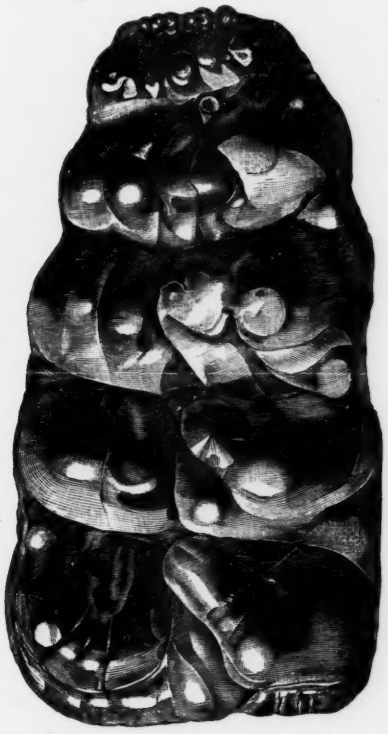


Fig. 3.—Last or Sixth Upper Molar Tooth of a Mastodon; half natural size.

the veteran Elephant hunter, Sir S. Baker, in his "Wild Beasts and their Ways," making the statement in regard to Elephants that "both the Indian and African varieties have only four teeth," whereas, as a matter of fact, every Elephant has, during its whole life, six molars on each side of both the upper and lower jaws. Instead, however, of

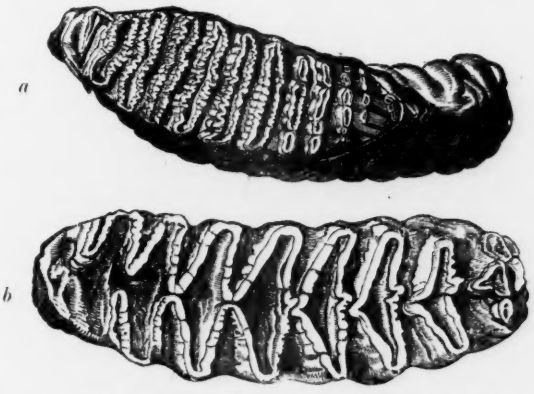


Fig. 4.—Molar teeth of Indian (*a*) and African (*b*) Elephant. In *a* the anterior half is worn, and the remainder unworn. Much reduced. After Owen.

all these teeth being in use at one and the same time, as are those of a cow or a horse, in existing Elephants there are never more than portions of two teeth in use at any one time, although, in the extinct Mastodons, portions of three may co-exist. In all Elephants, the more anterior of these six teeth are smaller and of simpler structure than the hinder ones, and the whole series is protruded in an arc of a circle from back to front, the larger hind teeth being pushed up and gradually coming into use as the small anterior ones are worn away and finally discarded. The hinder ones of these teeth (Fig. 4) are so large that while the front portion is being worn away the back is still bedded in the gum. In the earlier Elephants, or Mastodons, these molar teeth are composed of a series of relatively low and widely separated transverse ridges, more or less completely divided into inner and outer moieties, and with large open valleys between them. Moreover, in all the teeth, except the last, these ridges do not exceed four in number, although in the sixth tooth (Fig. 3) they may be as many as five or six. It was explained in the article on teeth how such a molar tooth might have been derived from the ordinary type of tooth presented to us by the molars of the pig, in which the crown carries four columns, severally placed at its corners. When a tooth like the one represented in Fig. 3 became worn down by use, the enamel coating each of the columns would be cut through, so as to expose a series of more or less trefoil-shaped surfaces of ivory, each surrounded by a ring of the hard enamel. And it will be obvious that a tooth thus constructed of alternations of substances of different degrees of hardness would act as an efficient mill-stone. Elephants do not appear, however, to have been by any means satisfied with this comparatively simple kind of tooth, for as we pass upwards in the geological scale we find that there has been a gradual increasing complexity in the structure of the molar teeth of these animals, this being due to a graduated increase in the height of their transverse ridges, accompanied by an increase in the number of the ridges themselves. There is, indeed, an almost perfect structural gradation, now known to exist between the Mastodon tooth, represented in Fig. 3, to the teeth of true Elephants shown in Fig. 4. Both the latter examples are in a somewhat worn condition, but it will be readily seen that the lozenge-shaped surfaces of ivory, surrounded by enamel, in the tooth of the African Elephant, correspond to the transverse ridges of the Mastodon's tooth. In the true Elephants, however, the open valleys between these ridges (which have now assumed the form of tall, thin, and nearly parallel laminæ) have been completely filled up by a third constituent of the tooth, known as the cement. The grinding surface of the tooth of such an Elephant consequently consists of a solid mass, made up of alternating vertical transverse layers of various substances, arranged in the order of cement, enamel, ivory, enamel, cement; and since each of these constituents differs in hardness, it will be obvious that the millstone-like apparatus is now of a far more efficient type than it was in the Mastodon. Moreover, as the crowns of the molars of the true Elephants are very much taller than are those of the Mastodons, it is evident that they will require a longer period of time before they become worn away, and that they will therefore allow a longer life to their owner. Even, however, among true Elephants there is a considerable amount of difference in regard to the number and narrowness of the component plates of their molar teeth, and it will be seen from Fig. 4, that in this respect the African Elephant departs far less widely from the Mastodon than does its Indian cousin. Since the food of the latter consists to a large extent of boughs and twigs, while that of the former is composed more of juicy leaves, fruits, and roots, the necessity of a more complicated

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masticating apparatus in the one than in the other is apparent.

In modern Elephants, the six molar teeth on either side of each jaw are all that are ever developed. This is, however, by no means the case with some of the earlier Elephants, and with most of the Mastodons. In these animals, when the second and third molars became worn out, they were succeeded vertically by much smaller teeth, from which we learn that the first three molars of the modern Elephants really correspond to the milk-teeth of other Mammals, which, as we all know, are succeeded vertically by some of the teeth of the permanent series.* This succession of the teeth shows us, therefore, another point in which Mastodons tend to connect modern Elephants with ordinary Ungulates.

We might go further and enter upon the consideration of some of the structural peculiarities presented by the soft parts of Elephants. Enough has, however, been stated to show that while these animals have preserved a very ancient type of structure in their limbs, they have acquired a very special modification in the structure and mode of succession of their teeth. And it is highly probable that it is owing to this particular specialization that Elephants have survived to our own day, while all the other plantigrade and five-toed primitive Ungulates have completely passed away; while it is almost certain that it is this feature alone which has enabled them to attain the gigantic bulk which forms one of their most striking features. In regard to their evolution perhaps no group shows more clearly than that of the Elephants how exceedingly important is the study of fossils to elucidate the relations of existing animals. Had we only the two living species of Elephants to deal with, we should never have had the least inkling of the manner in which they were related to other Ungulates, imperfect as our knowledge of the relationship still is. Moreover, from the distribution of these two species, it would have been naturally inferred that Elephants were creatures suited solely to tropical or sub-tropical climates. The discovery of frozen carcases of the Mammoth—a species closely related to the Indian Elephant—in the Siberian "tundras" shows, however, that this animal (Fig. 1) was suited to dwell in at least comparatively cold regions, although it is probable that the climate of Siberia was formerly not so rigorous as it is at present. To withstand the cold of these northern regions the Mammoth was protected by a coat of long reddish hair, beneath which was a shorter covering partaking more of the nature of wool. Along the borders of the Arctic Ocean for hundreds of miles Mammoth remains are met with in incredible quantities; and it is still one of the puzzles of geology to account adequately and satisfactorily for the manner in which these creatures perished, and how their bodies were buried beneath the frozen soil before decomposition had begun its work, for it is hardly possible to believe that they lived in a climate so rigorous that their bodies would have been frozen on the surface of the ground immediately after death.

Another rude shock to our common ideas of Elephantine nature is afforded by the extinct Elephants of Malta, which show us that gigantic size is not a necessary concomitant of the group; and that when the area in which a species dwelt was small, the size of the species itself was proportionately reduced, These little Maltese Elephants were very closely allied to the living African species, but whereas "Jumbo" attained eleven feet in height, and wild specimens of the African Elephant may be still larger, the smallest of the Maltese species was scarcely taller than a donkey. So small, indeed, are the bones and teeth of this species exhibited in the Natural History Museum, that it is sometimes difficult to convince people that they really belonged to Elephants at all.

As regards their distribution, Elephants and Mastodons formerly roamed over the whole world with the exception of Australia; true Elephants ranging over the whole of the Northern Hemisphere, while Mastodons extended as far south in the New World as the confines of Patagonia. It is in the north-east of India, Burma, and the islands of the Malayan region that the fossil Elephants connecting the living species with the Mastodons are alone found; and it is thus probable that from these regions the true Elephants migrated westward into Europe and Africa, while the Mammoth in later times crossed from Asia into Alaska by way of Behring Straits. That the Mammoth, which ranged from the Arctic regions to the Alps and Pyrenees, was a contemporary of the primeval hunters of Europe is now a well-established fact, but it appears that throughout the Old World Mastodons had utterly died out before the advent of man. In the New World, however, the continuity between the old and the new fauna was more fully sustained, the Missouri Mastodon having survived well into the human period, so that we have in this survival a good instance of the vast changes that have taken place in the fauna of the globe within what we may metaphorically call the memory of man.

* See article on "Teeth and their Variations."

THEORIES OF GLACIER-MOTION.

By the Rev. H. N. Hutchinson, B.A., F.G.S.

THERE are few subjects in physical geology which have excited more interest than that of the motion of Glaciers. Ice is a very peculiar substance; and some of its properties appear so contradictory to others, that scientific men of high attainments have been greatly puzzled by its behaviour as manifested in Glaciers. Hence when we come to consider how, or why, Glaciers flow down their valleys, we find a great diversity of opinion. Readers of Knowledge may therefore be glad to have a brief account of the different theories of Glacier-motion which have from time to time been brought forward, together with some indication as to their relative merits.

First, with regard to the origin of the ice itself; most of our readers will be aware that Glaciers are fed by the snow-fields above them. So it may almost be said that a Glacier is snow at one end and ice at the other. How then does the change from snow to ice take place? Pressure, as every school-boy knows, will convert a handful of newly-fallen snow into a hard mass, and if the pressure be continued, the hardened snow will become ice. It is partly this property of snow which makes a Glacier, or ice-river possible. When snow has accumulated to a considerable depth, its own weight squeezes down its lower strata; and the underlying portions of snow are finally compacted together until they become true ice. But another cause is at work helping to bring about the same result. By day, when the sun shines upon the snow, or warm air passes over its surface, the surface-layer gets partially melted, the water thus formed trickles down into other snow below, and there solidifying, when night comes and a fall in temperature takes place becomes part of the great mass of ice. Summer and winter act in the same way as day and night, so that much of the winter's snow gets melted and turned to ice. Thus, partly by thawing and freezing, and partly by pressure

(but chiefly by the latter) the snow of the higher regions becomes the ice of the Glacier.

It might be supposed that such an apparently hard and brittle substance as ice would refuse to move downhill, and hence it is by no means easy at first to understand how ice can flow down valleys as it does. The mean daily rate of movement of the *Mer de Glace* (in the centre) during the summer months, is as much as 20 to 27 inches. The question is—how is such a flow to be accounted for?

In the different theories of Glacier-motion that have from time to time been brought forward, some account for the flow solely by gravitation, ignoring the fact that ice is not a truly rigid body; others introduce melting, or melting and freezing; and one brought forward by J. D. Forbes attributes the river-like flow of the ice to a plastic power in the ice itself, as if it were a viscous or semi-viscous substance. These theories may be briefly indicated as follows:—

1. The celebrated *De Saussure*, a pioneer in Alpine work, whose book, "Travels in the Alps," is full of original observations, conceived that the weight of the ice might be sufficient to urge it down the slope of a valley if the sliding motion were aided by water flowing at the bottom. He regarded a Glacier as a rigid mass capable of sliding down an inclined plane just as any solid body might—for instance, as a slate when loosened slides down the roof of a house. There are many objections to this simple theory; one is that there ought to be accelerated motion, which there is not. Besides, De Saussure was ignorant of certain important facts, to be noticed presently. So we may dismiss this theory.

2. *Hopkins' Theory.*—Mr. Hopkins, a well-known Cambridge mathematical coach, who applied his knowledge to several important geological problems, put forward a theory which may be described as follows: He contended that a Glacier can move along a very slight slope solely by gravitation, owing to the constant dissolution of ice in contact with rock below, and the number of separate fragments into which the Glacier is divided by fissures, so that freedom of motion is imparted to its several parts, somewhat resembling that of an imperfect fluid. His argument was supported by a number of ingenious experiments. He found that ice will move down a very slight slope, even a slope that the eye could not perceive. This theory was very similar to that of De Saussure, only he added to it the idea of a Glacier being broken by frequent fissures into separate pieces. It is needless to say that this theory is out of harmony with the facts.

3. *Charpentier* substituted for De Saussure's sliding theory an ingenious explanation, which may be called "the Dilatation Theory." The most solid ice is always permeable to water, and penetrated by innumerable fissures and capillary tubes, often extremely minute. These imbibe water (due to melting) by day, which freezes during the night, and, of course, expands in the act of congelation. This was supposed to cause a distension of the whole mass, tending to propel the Glacier in the direction of least resistance, namely, forwards. Mr. Hopkins opposed this theory in several able papers. He contended that the distension—if it existed—would tend to act upwards, and increase the thickness of the Glacier, rather than downwards, or in other words, down the valley.

This theory has been demolished in several ways. In the first place, coloured fluids have been injected to see whether the said capillary tubes existed; but they have never been detected. Again, a Glacier should, on this theory, move faster about the time of sunset, when the freezing of the water must be greatest. But this is not the case.

4. *Mozeley's Theory of Expansion and Contraction.*—Canon Mozeley noticed that the lead on some parts of the roof of Bristol Cathedral kept gradually crawling downwards, tearing up its fastenings in the act. This fact seemed very remarkable, until he explained it by showing that during the day (the temperature being higher) expansion took place, while during the night contraction took place. Both these would take place chiefly in the direction of least resistance, namely, downwards. He then applied this explanation to the downward movement of Glaciers. This theory was ingenious, but like the others, fails to explain all the facts.

5. *Prof. James Thompson's Theory* accounts for Glacier-motion in the following manner:—The freezing point of water is affected by pressure, and relaxation of pressure will cause the water at the bottom of a Glacier to freeze, and a renewal of the pressure will cause it to thaw. His idea is that the pressure due to the weight of a Glacier thaws the ice at the bottom, and that this thawing enables the Glacier to glide downwards (by diminishing friction). Then the relaxation of pressure that follows the down-sliding causes renewed freezing until once more the Glacier's own weight brings about another melting. This theory is also unsatisfactory.

6. *Croll's Molecular Melting Theory.*—This ingenious theory by the late author of "Climate and Time," is rather too subtle. Briefly, he supposed that the ice is melted molecule by molecule, each molecule becoming, for a time, changed into the liquid state, and while liquid descending; thus a flow of heat was supposed to take place through the whole mass. This is how he accounted for the apparent viscosity of ice in Glaciers.

There are now only two theories left, each of which has powerful advocates now. Some think both are true. The first is—

7. *Tyndall's Regulation Theory.*—Prof. Tyndall believes that a Glacier bends sharp turns by splitting up and freezing together again. His theory is based on Faraday's well-known discovery of regelation, a principle by which when two pieces of melting ice are brought into contact, they will freeze together. The principle of viscosity, so admirably worked out by Forbes, he considers, will only account for a part of the facts. He admits that ice behaves as if it were a viscous substance when it is subjected to *pressure alone*, but when *tension* comes into play, he thinks the analogy with a viscous body ceases. His object is to reconcile the apparent brittleness of ice (for it is decidedly brittle in small blocks) with its power of turning corners, and other facts that seemed contradictory to the idea of brittleness and rigidity. In "Heat and Mode of Motion" he says, "The Glacier widens, bends, and narrows, and its centre moves more quickly than its sides." A viscous mass would, undoubtedly, do the same. But the most delicate experiments on the capacity of ice to yield, to strain—to stretch out like treacle, honey, or tar—have failed to detect this stretching power. "Is there," he asks, "then, any other physical quality to which the power of accommodation possessed by Glacier ice may be referred?" He believes regelation is the required principle, and that the mass of ice in a Glacier moves down the valley by a process of alternate rupture and healing. The gist of the regelation theory is that the ice of Glaciers changes its form and preserves its continuity under *pressure*, which keeps its particles together. But when subjected to *tension*, sooner than stretch it *breaks*, and behaves no longer as a viscous body.

8. *Forbes' Viscous Theory* is opposed by Tyndall, but has many advocates of authority. Principal J. D. Forbes discovered, by a series of measurements, that an ice-stream

moved slower at the sides than at the centre, and faster in the middle, as well as more rapidly at the surface than at the bottom. Consequently he proposed the theory that ice is a plastic substance, capable of yielding to great pressure, and the more so as it approaches the melting point. This theory was not supposed to be irreconcilable with the fact that it will crack under considerable pressure. In small masses this plasticity is not noticeable, but in large masses, and under long-continued pressure, it slowly yields, and will flow like a stiffly viscous fluid. In large masses like a Glacier, this steady, powerful pressure is furnished by the immense weight of superincumbent ice.

Many persons consider that both Tyndall and Forbes' theories are true, and so combine the two; but to the writer it seems that the plasticity of Glacier-ice under great pressure is so well proved as to render the regelation theory almost unnecessary; and according to it, there ought to be more cracks and crevasses all over Glaciers, and not only in their steeper parts.

THE GREAT SUNSPOT AND ITS INFLUENCE.

By E. W. Maunder, F.R.A.S.,

Assistant superintending the Solar and Spectroscopic Departments at the Royal Observratory, Greenwich.

IT is a strange circumstance that we seem to have scarcely any observations of Sunspots previous to the invention of the telescope. Only some three or four have come down to us; yet spots large enough to be seen by the unassisted eye are not at all infrequent at the times of special solar activity, and from time to time spots occur which are not only visible, but which are even conspicuous.* The writer well remembers when a boy at school, and not in the least likely to have been on the look-out for such an occurrence, having been surprised by the sight of an intensely black speck on the setting sun. On a more recent occasion, when the Queen was holding the review of the troops who had returned from the Egyptian campaign, the morning was a foggy one, and no dark wedges or smoked glasses were needed to tone down the brilliancy of the sun to the eye. It shone feebly through the fog, a dull red ball, but not, as usually, with unvaried surface; a big black blot was conspicuous upon it, so conspicuous as to attract the attention of the soldiers who were crossing Blackheath on their way from Woolwich to Hyde Park to attend the review, and called forth much comment and speculation from them. It may, therefore, be safely assumed that the object on the sun was something not easily overlooked, and it is hard to understand, if spots of similar magnitude have occurred upon the sun from time to time, during the ages which preceded the days of Fabritius and Galileo, how they escaped notice.

But the great Sunspot of the February just past fairly dwarfed that of November, 1882, to which I have referred above, and whenever the sun was sufficiently dull for the eye to be able to look at it without pain, it was not merely an easy but a conspicuous object. Nor can we wonder that it was so when we come to consider its real dimensions. For on February 13th the spot had an extreme length of 14° of solar longitude, and a breadth of 8·2° of solar latitude, equivalent to 92,000 and 62,000 English miles respectively; whilst the entire group of which it formed the principal part was nearly 25° in length, and 10° in breadth, or in miles 162,000 and 75,000. The area of the great spot on this day reached the astonishing amount of 2940 *millions* of square miles, or reckoning in the smaller spots which clustered so thickly round the central one, the spotted area of the whole group was 3530 millions of square miles. Such an area is all but 18 times that of the entire surface of the terrestrial globe; or to put the matter in another way, some 70 worlds as large as our own could have lain side by side in that immense hollow.

These figures may seem incredible until we recall how vast is the scale upon which the sun itself is constructed. Thus the photographs of the sun taken at Greenwich, one of which is reproduced in the accompanying plate, are on a scale of 8 inches to the solar diameter, a sufficiently large scale to show much detail. Yet a spot with an area of one million square miles would have a diameter of but one hundredth of an inch on such a photograph; practically it would be the smallest object which would be measured; any smaller spot would be quite a negligable quantity. A spot of *only* one million square miles area may be taken as the minutest object that can be satisfactorily dealt with on such a photograph.

We might look at our giant spot from another point of view. Suppose that an object of the same intrinsic brightness as the sun, and of the same apparent size as the great spot, were shining in the midnight sky, how would it appear to us? If we give the sun's "magnitude" as 26½ times brighter than an average first magnitude star, then from such a body as I have supposed we should receive more light than from 130 million stars as bright as Procyon, more light than from 3680 full moons. The planet Uranus is sufficiently bright to be detected by the unaided eye on a clear and steady night by any keen-sighted person, yet the light by which it shines is practically all derived from the sun, which, as seen from Uranus, is not nearly as large as this great spot appeared to us. Even Saturn, bright and readily seen as that planet is, does not receive much more than twice the light from the sun which a region of the sun equal in area to the Sunspot radiates to us.

If, therefore, the Sunspot were absolutely black, its presence on the sun would imply that the light of the sun was diminished by the amount of 3680 full moons, or 130 million stars as bright as Procyon.

Now it is true that the spot was very far from being absolutely black. The greater part of its surface, the penumbra, was dark only by comparison with the greater brightness of the sun; and even the darker portions, the umbra, still radiated a large amount of light. For it has been found perfectly easy to observe the spectrum of the darkest portion of Sunspots, which affords clear proof not only that they radiate some light, but that they radiate a great deal. Still, if we assume the penumbra to be as much as two-thirds as bright as the general surface, and the umbra as one-quarter as bright, we have a loss of light due to the spot of some 1455 full moons; a loss much greater than the total light Neptune receives from the sun, and but little less than Uranus receives. But if we take the figures Sir W. Herschel gives, and regard the brightness of the penumbra as 47 per cent. that of the general disc, and of the nucleus as only 0·7, then our Sunspot becomes responsible for a loss of light equal to 2200 full moons. This is three times as much light as Neptune

* The late Mr. John Williams, formerly Assistant Secretary of the Royal Astronomical Society, collected several Chinese accounts of Sunspots, which had been observed by the naked eye. In the *Monthly Notices* for April, 1873, he gave a list of such observations, extending from A.D. 301 to A.D. 1205. Some of the spots were described as "like a hen's egg," "like a duck," "resembling a large date" Possibly our foggy atmosphere, which often enables us to look steadily at the setting and rising, and sometimes at the meridian sun, may account for the fact that spots visible to the naked eye have frequently been observed in England, while, as far as I am aware, the Greeks and Romans did not notice them.—A. C. Ranyard.

receives, considerably more than Uranus receives, and is much more than one-fourth of that which falls upon Saturn.

These figures, of course, represent not the average size of the group, but its greatest size. It would appear to have been quite as large, perhaps even larger, on February 11th and 12th as on February 13th, the date for which the preceding figures have been computed. But it had passed through many changes before it attained so great a development. We cannot trace it back to its formation, for that took place upon the further side of the sun, and it yet remains to be seen whether it will end its course in the visible or the invisible hemisphere. The first time it came under observation was November 15th, 1891, when it was seen at the eastern edge of the sun. It was already a fair-sized spot, with an area of about 470 millions of square miles when first seen, and it increased in size from day to day. At this time, November 19th and 20th, it resembled the general type of the more important spot groups in its appearance, that is to say, it was divided into three principal regions. The largest spot of the group led, then followed a few small scattered spots, and the procession was closed by a large spot of irregular outline. Usually the leading spot in such a group tends to become very well defined in outline and circular in shape, and it increases in size, whilst the rest of the group tend to die out. It was not so with the present group. After passing the central meridian of the sun great changes were seen, in which the following spot assumed the circular form of the first, and the leader broke up into a chain of small spots. This chain, however, conformed to the typical spot history, and before the group had been carried out of sight round the western edge of the sun, it had become reduced to a single circular spot, making with the following spot a pair very similar in appearance and size.

The group was next seen at the eastern edge on December 12th, and remained visible until December 24th, returning into view on January 9th, and disappearing at the western edge for the third time on January 21st. No very noteworthy changes occurred during these two appearances. But when the group reached the east limb for the fourth time, on February 5th, it was altogether changed in appearance and size. It was now five times as large as when it had been last seen on January 21st, and it was to increase still further in size. The distressingly bad weather we had in England during the next week prevented further observations at Greenwich until February 13th, the date upon which the photograph was taken which is reproduced in the accompanying plate. From this date it diminished in area somewhat until February 18th, when it reached the western edge. It returned to our view on March 4th, shrunk in size beyond all recognition. The entire area of the group was now only about 250 millions of square miles, and of the largest spot only 120 millions. It revived a good deal during the following days, but though it attained very important dimensions, and showed several interesting changes, it at no time approached the magnificent proportions it had exhibited during February. It reached the western edge on March 17th, and if it still continues in existence may be expected to reappear again on the eastern border on March 31st or April 1st.

It was, therefore, only during a small portion of its life that the group attained the enormous development shown on our photograph. It happened with this spot, as it often happens with other large groups, that its greatest development took place very nearly at the time when the rotation of the sun brought it nearest to the centre of the disc. The days, therefore, when the actual area was greatest were those when the effect of foreshortening in diminishing the apparent size of the group was least; so that it was seen most nearly in its true proportions just at the time when these were at their maximum.

It was, therefore, only for three or four days that the spot was effective in diminishing the sun's light to the extent we have mentioned. Still that diminution would appear to be so serious in actual amount that even if we regard it as lasting but three days it would seem reasonable to expect that it would make itself felt here. And certain effects were undoubtedly experienced, though perhaps not those which might have been naturally looked for, and though it is not by any means certain that they had any direct connection with a diminished radiation from the sun, it might, perhaps, have been supposed that a loss of light equivalent to that afforded by over 2200 full moons would have seriously affected terrestrial weather. It is possible that it may hereafter be proved that it has such an effect, but up to the present nothing of the kind has been satisfactorily established. And even supposing, what is quite possible, that the diminution of radiation over the spot itself is not compensated by an increased radiation from other parts of the surface, it must be fully borne in mind that the greatest loss due to the spot must be but a very small fraction of the total solar radiation. The difference due to the greater distance of the earth from the sun at one part of its orbit far outweighs it. For the extreme difference due to that cause is one part in fifteen, but the greatest difference which, on the assumptions we have made, we can ascribe to even such a spot as that of February 13th would be no more than one part in 320, or only three times the effect produced by Venus in transit. Any *direct* effect upon the weather, therefore, must, in any case, be of the slightest, the more so that Sunspots are short-lived, and the time during which they present their full face to the earth is necessarily limited to three or four days.

But if the weather shows little or no sign of Sunspot influence, that influence does really exist, and makes itself unmistakably evident; and our February Sunspot did not fail to make its own individual record. Down in the cellars of the Royal Observatory, Greenwich, and the Kew Observatory, and at many other similar institutions all over the world, delicate magnetic needles are kept carefully suspended so as to be free to move at the slightest impulse. The position which such a needle takes up in England is neither a horizontal nor vertical one, nor does it point truly to the geographical north. This deviation from the true north is what is known as the magnetic declination, or the ordinary "variation of the compass." But such a needle does not remain stationary in the position it takes up. Day by day it appears to make an effort to follow the sun as it crosses the sky. The greatest deviation from the mean position towards the west takes place about two o'clock. The needle then begins to move back again, and by about ten o'clock at night has returned to its average position. There is usually but little movement during the night.

This curious daily motion varies in character and extent at different times. Notably it varies with the season of the year, but for our present purpose its variation from year to year is more important. For it has been clearly established that, *when we take average results for successive years*, we shall find that this motion is greater in amplitude and force in strict proportion to the number and size of the spots upon the sun.

That there is this *general* connection between the magnetic variations and Sunspot frequency is admitted. But when we come to the question as to whether individual

spots are able to exercise a special and individual influence, we find there is still a difference of opinion. We may therefore hopefully look to the record of spots like that of November, 1882, or of the February just past, for information on this point, for we naturally expect spots so gigantic in size will be the most powerful in influence.

(To be continued.)

ON THE CONNECTION BETWEEN SUNSPOTS AND MAGNETIC STORMS.

By A. C. Ranyard.

THE lower picture on our Plate represents a large Sunspot, photographed by M. Janssen, on a scale of nearly five feet to the sun's diameter. Its irregular form is characteristic of a period of sunspot maximum disturbance such as we have now entered upon. During such periods the willow-leaf or rice-grain structure of the photosphere becomes rounded and less strikingly distinct than at a period of minimum sunspot development. At all times a mottling of the photosphere is recognizable, produced by the haziness or smudged appearance of the grains or cloudlets in certain areas, while between the hazy areas the brilliant little cloudlets and the small black spaces between them are more distinctly visible, and are seen to be arranged more or less parallel to one another. As a period of sunspot maximum arrives, the character of the whole photosphere seems to alter. The change is not merely confined to the latitudes within 40° of the solar equator, where the spots appear, but extends to the neighbourhood of the solar poles. Similarly the prominences which appear in all heliographic latitudes from the poles to the equator change their character.

We are at present only in a position to observe and collate facts, and we seem to be very far from understanding the great periodic changes going on before us.

There is evidently a close connection between the development of spots on the sun's surface and the swaying of the earth's magnetic axis. More than one popular writer has spoken of this connection as proving that the sun is magnetic; and that solar storms sway its magnetic axis—and, further, that every motion of the great solar magnet is accurately followed by a corresponding motion of the magnetic axis of the planets, which bow and swing, always keeping parallel with the axis of the great central magnet.

But the earth's magnetic axis revolves about the earth's axis of rotation once in twenty-four hours describing a circle amongst the stars of nearly 20° radius. If then, the earth's magnetic axis and the sun's magnetic axis were permanently parallel, we should have to assume that the sun's magnetic axis travels round a line which is not the sun's axis of rotation in a period equal to the earth's period of rotation, which seems highly improbable.

There is considerable difficulty in conceiving of a hot gaseous body like the sun being magnetic. The difficulty occurred to Sir Isaac Newton, who, in a letter written on the 16th of April, 1681, wrote: "Concerning the experiment that a magnet loses its magnetism by heat, some have indeed supposed the sun to be cold, but I believe Mr. Flamsteed is not of this opinion, for they may as well affirm culinary fire to be cold. For we have no argument of its being hot, but that it heats and burns things that approach it, and we have the same argument of the sun being hot. Were we ten times nearer him, no doubt, we should feel him a hundred times hotter, for his light would be a hundred times more constipated, and the experiment of the burning-glass shows that his heat is answerable to the constipation of his light. . . . . The whole body of the sun therefore must be red hot, and consequently void of magnetism, unless we suppose its magnetism of another kind from any we have, which Mr. Flamsteed seems inclined to suppose."

It is possible that though the sun itself may not be magnetic, it may act as a magnetic body because it is surrounded by a magnetic envelope or region where its gaseous constituents are precipitated into solid or liquid magnetic particles. During the past year Professor Dewar has shown that oxygen becomes strongly magnetic when liquefied at a temperature of −180° Cent. The vapours of iron when precipitated in the comparatively hot lower regions of the corona, would also form a cloud of magnetic fog or dust. There is some evidence, in the forms of the coronal streamers seen in the neighbourhood of the sun's poles, that the coronal particles are magnetic, and tend to arrange themselves along lines of force, as if the whole sun had a magnetic axis, nearly but evidently not accurately, coincident with the sun's axis of rotation.

The corona is far from being accurately symmetrical with respect to the sun's axis of rotation; it is denser in parts, and has projecting rays or structures which extend to a great distance from the sun, especially in the sun's equatorial regions. On the above theory we should expect to find the magnetic region similarly unsymmetrical, and a body passing round the sun, near to the plane of the solar equator, would be subject to very unequal disturbance from the magnetic particles of the corona. This seems to tally with the facts observed—for the greatest magnetic storms have generally taken place when a large spot has been seen near to the centre of the sun's disc. We know very little at present as to the connection between the corona and sunspots, or as to how far the corona extends—some of its larger structures may extend as far as the earth's orbit, or as far beyond our orbit as the zodiacal light extends. There is no evidence that large coronal structures exist over large sunspots, but there is evidence of an intimate connection betwen the general development and arrangement of the parts of the corona and the spottiness of the sun's surface, as well as between the development of large prominences and sunspots.

The sudden manner in which these magnetic storms commence seems rather to indicate that the earth plunges into a magnetic or auroral region, than that the magnetic equilibrium of the whole solar system is suddenly disturbed. There is no brewing of a magnetic storm, it breaks out with full violence from its commencement.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

STELLAR SPECTRA.

To the Editor of Knowledge.

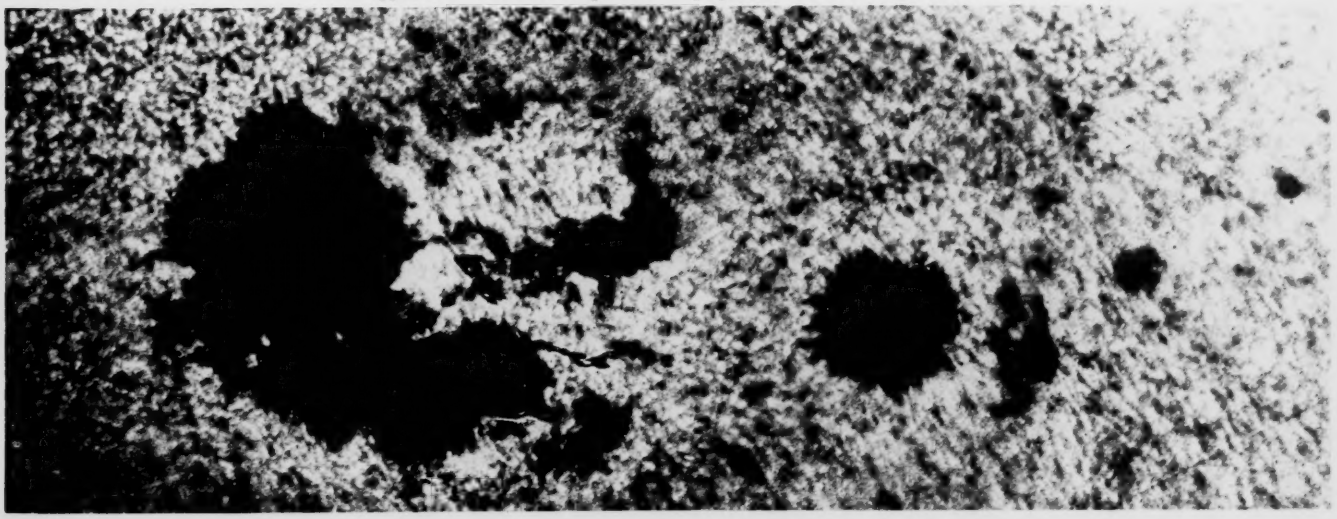
Sir,—With reference to Mr. Monck's remarks on the spectra of binary stars in Knowledge for March, 1892, O Σ 4 seems to be identical with Lalande 220. I find that the star in the Draper catalogue is identical with Lalande 221, which lies about 8′ 25″ north of the binary. Both stars were rated $7\frac{1}{2}$ magnitude by Lalande, and are 8 magnitude in Harding's atlas. They lie closely *south preceding* the $4\frac{1}{2}$ magnitude star σ Andromedæ. The spectrum of O Σ 4 was sent to me by Professor Pickering.

With reference to μ^2 Bootis my authority for its spectrum is Professor Pickering, who kindly sent me the

XUM

PHOTOGRAPH OF THE GREAT SPOT ON THE SUN.

Taken at the Royal Observatory, Greenwich, on the 13th February, 1892, at 9h. 47m. 18s. The Sun's axis is placed vertically on the page, with the North Pole uppermost. The wires are parallel and perpendicular to the Earth's axis.



LARGE PHOTOGRAPH OF A SUNSPOT OF MAXIMUM TYPE.

Taken by Dr. JULES JANSSEN, on the 1st June, 1881.

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spectra of several binaries by private letter in July, 1890, when I was compiling my catalogue of binary star orbits.

Yours faithfully,

March 18th, 1892. J. E. Gore.

PERIODICAL COMETS DUE IN 1892.

To the Editor of Knowledge.

Sir,—In reference to my short article on this subject in your February number, will you allow me space for a few words to mention that M. Gautier has written a letter to the *Observatory* pointing out that I had overlooked an investigation of his own (published in No. 2656 of the *Astronomische Nachrichten*) on the motions of Tempel's first periodical comet, which shows that, in consequence of great perturbations produced by the action of Jupiter upon it between 1879 and 1885, the duration of its revolution was increased by some months, so that the last return was not due until the autumn of 1885, and another may be expected in the month of April next. That comet was discovered by Tempel at Marseilles, on the 3rd of April, 1867, and was observed at the returns of 1873 and 1879, but has not since been seen. In all those years it passed its perihelion in May, but, according to M. Gautier's calculations, the subsequent lengthening of its periodic time was such that the next perihelion passage probably occurred on the 25th of September, 1885. It was not, however, seen, he remarks, on that occasion "par suite d'un concours de circonstances défavorables." But, as it has kept at a respectful distance from Jupiter since 1885, there is every reason to suppose that it has suffered no further important change in its period, and that it will return to perihelion in the early days of April in the present year.

Yours faithfully,

Blackheath, February 5th, 1892. W. T. Lynn.

ON THE CAUSE OF EARTHQUAKES.

To the Editor of Knowledge.

Dear Sir,—Nearly forty years ago a tremendous explosion occurred at Gateshead, during a great fire. It shook the whole of Newcastle, and was heard at Tynemouth, and ten miles out at sea. Burning rafters were carried high into the air; some of them falling on the other side of the Tyne caused a fire in Newcastle, and one rafter fell in my works, on the Gateshead side of the river, three-quarters of a mile away from the scene of the explosion. I visited the ruins and saw that the *débris* was arranged in the form of a crater, and that inside the crater there was a pool of water thirteen feet deep.

The fire and explosion had been the cause of several deaths, and a very searching enquiry was made at the inquest as to the cause of the explosion. There was a general belief amongst the inhabitants of Newcastle that gunpowder had been stored in the warehouses, contrary to the law, and much indignation was expressed against the warehouse owners, but the evidence of Mr. Hugh Lee Pattison, which I enclose, goes conclusively to show that the explosion was due to the sudden generation of steam. I shall never forget the sensation produced at the inquest when the witness said, "To produce an explosion another element is wanted." He paused, and everybody expected the word "gunpowder" to follow. But he went on, "and that is water," and he ultimately succeeded in convincing the jury that the water they had looked upon as an element of safety was more dangerous than gunpowder.

It seems to me that if water can produce such a tremendous concussion by its sudden conversion into steam when it is confined by warehouse floors, with a few tons of goods upon them, its explosive force would be sufficient to shake the earth, and throw down buildings on the surface, if it were suddenly converted into steam at a great depth. I think that it is pretty generally admitted that volcanic eruptions are always accompanied by a great evolution of steam, which continues to issue and form a white cloud steaming away from the volcanic vent long after the first outburst has ceased; and it seems to me much more probable that the great shaking of the earth's crust, as well as the noises heard during some earthquakes, are produced by such explosions, rather than by the grinding together and slipping of strata along a line of fault, as has been suggested by modern theorists.

Yours faithfully,

George Crawshay.

FROM THE REPORT OF THE INQUEST.

Date of Explosion, October 6th, 1854.

"Hugh Lee Pattinson was then sworn and gave his evidence:—He said he had looked to the contents of the warehouse furnished him by the coroner, and was of opinion that not one of the substances was explosive by itself. He was also of opinion that not one of the substances would become explosive by being roughly mixed together. He thought he could go further and say that hardly any three of them would become explosive. During these two days and this morning, previous to this enquiry, he had made several experiments, not because he felt any philosophical doubt, but to make assurance doubly sure. He had melted nitrate of soda, and when perfectly fluid and red-hot he had poured it into melted brimstone, and there had been produced certainly intense heat, but no explosion. He had introduced into melted nitrate of soda, while red-hot, melted guano, melted zinc and coal tar, that was all the explosive materials, and there had been no explosion; there simply took place deflagration, which, according to Crabbe, meant a gradual sparkling combustion of any substance without violent explosion—a term particularly applied to combustion applied to nitre. It followed, therefore, that the contents of the warehouse alone would not explode. There wanted another element, and that element was water; for we had abundant evidence that when water came into contact with intensely hot and melted saline matter generally a violent explosion took place. This he had also proved by experiment, by introducing a small quantity of water into a crucible and also into a large jar, both containing incandescent nitrate of soda, deflagrating with sulphur, and in each case the vessel was shivered to pieces with a loud explosion, and he produced the pieces in court. Various other experiments of the same nature were made by himself and assistants, all with the same results, and, in support of the same view, he read from the London, Edinburgh, and Dublin 'Philosophical Magazine' for 1850, a paper by Dr. Hare, of Philadelphia, detailing experiments which showed that an explosion similar to the present, which occurred in New York in 1845, had occurred from this cause. Mr. Pattinson went on to say that the fire, the effects of which they were considering, had burned a considerable time before the explosion, and the evidence showed that towards the base of the building, in what was called the vault, there were 47 tons of sulphur spread out on the floor, over which was placed a tarpaulin cover, and upon the top of the cover there were placed 45 tons of nitrate of soda in bags. Now, his opinion was that the sulphur in the vault took fire, which it would do at a temperature of 500 degrees or thereabouts. This would set fire to the bags containing the nitrate, and some of the nitrate would be melted, which, flowing over the burning sulphur, would produce most intense combustion, and this would, in a little time, extend itself to all the sulphur and nitrate in the vault. The large quantity of these substances thus deflagrating in a confined space would necessarily generate an intense heat, more intense, probably, than we could well conceive, and if at this time water in sufficient quantity should find its way into the vault by any means, it would come in contact with the highly incandescent salts, when steam of resistless force would be instantaneously generated, and this would occasion the explosion. It had been imagined that this explosion had been produced by eight tons of gunpowder, nobody imagining that more was required, and nobody talking of any less; but probably four tons of gunpowder would produce the explosion. Now, to compare the force of steam with that of gunpowder they had the following data:—Mr. Robins, who experimented in gunpowder some years ago, found that 27 grains of gunpowder, yielded an explosion of 346 cubic inches of permanently elastic gas, at which rate a grain of gunpowder would yield

1·28 cubic inches of gas. 'We find,' says the article, 'Gunpowder,' in the 'Edinburgh Encyclopædia,' 'that 126·7 of gunpowder generates 169·6 cubic inches of gas, from which one grain of gunpowder would yield 1·25 cubic inches of gas. This, he should assume, was correct. Then 253 grains of gunpowder (the weight of a cubic inch of water) would produce 316·25 cubic inches of permanent gas. But one cubic inch of water, expanded into steam, occupied the space of 1728 cubic inches; hence the elastic force of water was to that of gunpowder as 1728 to 316·25 or 5½ to one nearly. It would, therefore, require only 3·66 cwt. of water to do the work of one ton of gunpowder. To do the work of eight tons of gunpowder would be required 29·88 cwts. of water, or 328 gallons, or about six hhds. of 54 gallons each, or 52½ cubic feet of water. Now, there could be no difficulty in imagining that this quantity of water would get into the vault, and cause the explosion.'

Experiments at the Felling Chemical Works.

"On arriving at the extensive chemical works of Mr. Pattinson at the Felling, that gentleman at once proceeded to make experiments, in presence of the coroner, the jury and Captain Du Cane, in support of the evidence he had given on the preceding day. He first caused a metal pot to be inserted in the ground until its top was level with the surface, and having put into it 9 lbs. of nitrate of soda and 6 lbs. of sulphur, he ignited the mass; and then heating it to the highest possible degree of which it was susceptible, he poured into it about a quart of water. The effect was immediate explosion, accompanied by a loud clap, which would have been exceedingly perilous to any person in its immediate vicinity. The experiment was repeated under precisely similar conditions, and attended with a precisely similar result. A trial of the same kind, which had been made before the arrival of the coroner and jury, had been accompanied with the additional effect that the metal vessel, which was the subject of it, had a hole blown through it in two or three different places. A series of experiments was then made under slightly different conditions. The pot, into which the sulphur and nitrate of soda were put, was covered over the top with a large piece of thick metal of considerable weight, and above that again were placed several large pieces of clay and earth. It was deemed necessary to try this experiment in an open field, away from any dwelling-house, and which admitted of the spectators placing themselves at a safe distance from the spot. The materials were then ignited as before, and when in the incandescent state water was poured upon the mass down a spout; but the result was only a comparatively slight explosion, and which scarcely disturbed the iron and clods placed over the mouth of the vessel. Another experiment of the kind was made with the same result. At length, a trial having been made for a third time, but with this difference, that the vessel was covered over the top with another similar vessel, and that the water was poured upon the sulphur and nitrate of soda with greater rapidity than before, by slightly elevating the spout, the effect was to blow up the pot on the top into the air to a height of upwards of 70 feet, accompanied by a loud detonation. With this the coroner and jury seemed convinced that whether or not the premises in Hillgate contained gunpowder, they contained, at all events, elements as certainly explosive, and, perhaps, far more destructive."

N.B.—A large water-pipe inside the building appears to have burst; thus adding to the water from the fire engines.

[There is some difficulty in understanding how without a prior shock, or dislocation, water could in any quantities enter heated cavities in the earth. A little water percolating in would, if converted into steam, tend to drive back and check the entrance of more water from above; and to produce a great explosion a considerable mass of water would need to be admitted rather suddenly to a hotter region. The slipping of strata, or any sudden change along a line of fault might, no doubt, afford a way for such a down rush, but one has to account for the initial movement. If, as is so often assumed, there is an average rise of temperature as we descend within the earth, amounting to 1° Fahrenheit for every 50 to 60 feet, water would be raised to 212° Fahrenheit at a depth of much less than two miles (*i.e.*, somewhere between 7500 and 9500 feet). Water under pressure would glow with a red heat at a depth of ten miles, and cast iron would glow white, and be raised to a temperature which would melt it at the earth's surface, at a depth of 30 miles. But the ordinary assumption with regard to a rapid increase of temperature is founded upon observations made within such mere scratches in the earth's crust, that we cannot safely assume that the temperature will increase uniformly as we proceed to greater depths. Our deepest mines only extend to a depth of a little more than half a mile from the surface, while our deepest borings do not extend to four-fifths of a mile in depth. In entering this mere outer skin of the earth it is found that there are great inequalities in the observed rise of temperature in different regions of the globe, such as certainly would not exist if the increasing temperature were only due to the secular cooling of the interior of the earth from an originally-heated mass. Thus, according to Prof. Judd, at the great boring of Grenelle at Paris, the increase of temperature down to the depth of 740 feet amounted to 1° Fahrenheit for every 50 feet of descent, but from 740 feet down to 1600 feet the rate of increase diminished to 1° for 75 feet of descent. In the deep boring of Sperenberg, near Berlin, which attained the great depth of 4052 feet, the increase of temperature for the first 1900 feet was 1° Fahrenheit for every 55 feet, and for the next 2000 feet it diminished to 1° Fahrenheit for every 62 feet of descent; while in the deep well of Buda-Pesth a decline in temperature was actually found below the depth of 3000 feet. On the other hand, at Monte Massi, in Tuscany, the temperature was found to increase at the rate of 1° Fahrenheit for every 24 feet of descent, while in the Comstock Mine there is a rise of temperature of 1° Fahrenheit for every 45 feet of descent between 1000 and 2000 feet from the surface, and a similar rise for every 25 feet at depths below 2000. Such irregular increases of temperature seem to show that there are great local sources of heat, due possibly to chemical changes going on, or more probably to the great crushing and crumpling of strata along lines of weakness, which are always taking place; raising mountain ridges and depressing other regions as the external shell of the earth contracts by secular cooling upon the central mass. It is a fact well known to geologists, that rocks which appear to have been subjected to great movement and contortion also appear to have undergone great chemical change, such as would follow from their being subjected to a high temperature in the presence of water. With such evidence of stress and change going on within the body of the earth, we need not necessarily look to explosions as the cause of the tremors which occasionally agitate the earth's surface.—A. C. Ranyard.]

OUR NEAREST NEIGHBOURS AMONG THE STARS.

To the Editor of Knowledge.

Dear Sir,—The late Mr. Proctor pointed out long ago that the best test of the relative distances of the stars which we possess is their relative proper motions. Not of course that there may not be near stars, whose proper motions are small, because they are moving almost directly towards us or away from us, but that every star, with large proper motion, must be comparatively near, unless we ascribe to it an enormous velocity. This is true, even if the apparent proper motions of these stars are largely due to the sun's motion in space, for it is only on near stars that the effects of this motion will be easily perceptible. When travelling by train, the trees at the end of the next field appear to be moving rapidly, while the distant range of mountains hardly changes its apparent place. In like manner an extremely distant star will appear to be unaffected by the sun's motion, while a near one will be considerably displaced. This conclusion, moreover, is borne out by the results of spectroscopic research. The spectroscope reveals no extraordinary

velocity on the part of some stars as compared with others. This appears not only from the accurate results of Vogel, but from the more extensive but less accurate researches made at Greenwich and elsewhere. I doubt if there is a star observed half a dozen times at Greenwich which gives an average spectroscopic velocity of 80 miles per second, and the average results obtained at Greenwich are confessedly too large. Stars no doubt differ in velocity as well as in other respects, but not to such an extent as to deprive large proper motion of its value as a test of nearness. And an examination of any catalogue of stars, with large proper motion, leads to the same conclusion, by showing us that their real motions are comparable with the sun's motion in space. Thus, taking M. Bossert's Catalogue of Stars, with proper motions of half a second or upwards annually, and taking the right ascension of the point towards which the sun is moving as 270°, the effect of the sun's motion will be to diminish the right ascension of all stars in the second and third quadrants, and to increase the right ascension of all stars in the first and fourth quadrants. As a matter of fact I find that three-fourths of the stars in the second and third quadrants are moving in the direction of diminishing right ascension, and three-fourths of those in the first and fourth quadrants are moving in the direction of increasing right ascension. This is just the result which would be obtained if the sun's motion in right ascension was equal to the average motion of the stars in question. The effect in declination is also manifest, though not quite so well marked, and the result appears to be that the stars in M. Bossert's Catalogue are not on the average moving with much greater velocity than the sun.

But when we examine the spectra of these fast-moving stars, one important fact immediately occurs to us. Over the whole sky the numbers of stars with spectra of the first type (Sirian stars) and of the second type (Solar stars) are about equal; but when we examine M. Bossert's Catalogue of Stars with large proper motion, we find that there are about eight Solar stars to one Sirian. (Stars with other types of spectrum are almost wholly absent.) The sun thus appears to be one of a class or cluster of stars whose spectra are of a type similar to its own. And we begin to see, too, why the brightness or magnitude of a star affords so doubtful an index to its distance from us. *Ceteris paribus*, Sirian stars appear to be brighter than Solar stars, and a classification according to brightness or magnitude will prove to a large extent misleading so long as we omit the consideration of the spectra of the stars. The relative effect of the light of two stars on the eye is often widely different from the relative effect on the photographic plate, and a mere change in the structure of the eye might revolutionize the whole aspect of the heavens. We must, therefore, note the *kind* of light as well as the *quantity* before we found any inference as to distance on it.

Another test of distance occurs in the case of binary stars, which is worth examining. If two stars, which are separable by the telescope, complete their revolution in a moderate period, the inference is that they are comparatively near us; otherwise the mass of the system must be enormous. On the other hand, if the angular distance between the stars is not large and yet the revolution is very slow, the pair is probably at a great distance; otherwise the mass must be very small. I accordingly compared Mr. Gore's Catalogue of Orbits of Binary Stars with the Draper Catalogue of Stellar Spectra, and I found that the former catalogue contained 32 Solar stars to 11 Sirians. That this preponderance was due to the comparative nearness of the Solar stars rather than to their large masses, appeared from the fact that many of them had large proper motions, and that in more than one instance a sensible parallax (with the result that the mass was not very large) appeared to be determined by actual measurement. Such stars are α Centauri, 61 Cygni, 40 Eridani, η Cassiopeiæ, and 70 Ophiuchi. It might be thought, perhaps, that Sirian stars were, for some reason, less likely to enter into binary combinations than Solar stars. But this does not seem to be the case. On comparing the list in the "Handbook of Double Stars" with the Draper Catalogue, I found that the Sirian stars were quite as numerous as the Solar stars—perhaps a little more so. But when we turn to the double stars, whose orbits have been computed, the proportion drops at once from one-half to one-fourth.

The exact limits of this Solar cluster, and the number of stars comprised in it, remain to be determined, but one thing, I think, may be affirmed of it—that it has a northerly drift. Thus, when Professors Boss and Stumpe deduced the position of the apex of the sun's way from stars with large proper motions, they obtained a less northerly position than when they determined it from stars with smaller proper motions.* It might be supposed that this was due to the greater actual velocity of the stars with large proper motions which had the effect of disguising the sun's motion in space, but a comparison of M. Bossert's Catalogue with the more extensive catalogue of Mr. Main does not lead to that conclusion. The effect of the sun's motion in R. A. is more marked in the case of the stars with large proper motions, though that in N. P. D. is less so. The stars with large proper motions are not perhaps faster movers than their compeers, but they have a general drift, which others have not; though whether the more distant stars are moving indifferently in all directions, or have a general drift differing from that of the solar cluster, remains to be determined.

I do not think, however, that all the Solar stars, or even all the bright Solar stars, belong to the cluster or system of which I am speaking. Betelgeuse† and Rigel, for instance, have not only very small proper motions, but judging from the attempts which have been made to determine their parallaxes are nearly as remote as the fainter Sirian stars which abound in the region of Orion. Procyon, Capella, Aldebaran, and Arcturus (in spite of Dr. Elkin's insensible parallax of the latter, in which I have not much faith), on the other hand, probably belong to the Solar cluster. The motions of the stars in this system may be illustrated by supposing that all the elliptic comets which move round the sun were simultaneously visible. They would appear to be moving almost indifferently in every possible direction, yet, taken all together, there would be a general drift in the direction of the sun's motion. Apparent indifference of motion may in this way be combined with a general northerly drift.

Further light, however, may, I think, be thrown upon the structure of the Solar cluster by sub-dividing the Solar stars into two classes, which I propose to designate Capellan and Arcturian respectively, after their most brilliant representatives. The former class includes stars with the spectra denoted by E and F, but more especially F, in the Draper Catalogue. They are, on the whole, somewhat less numerous than the Arcturian stars, but of the 32 Solar binaries for which orbits have been computed, no less than 23 have spectra of the Capellan type, the spectrum in 20 cases being F. They actually outnumber all other binaries put together. Taking Mr. Main's Catalogue of Proper Motions and comparing it with the Draper Catalogue, I found that, roughly speaking, only 24

*See *Old and New Astronomy*, Part XII.

† But the spectrum of Betelgeuse is probably of the third type.

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per cent. of the Sirian stars had proper motions, amounting to one-tenth of a second annually (for the Orion stars, Spectrum B of the Draper Catalogue, the proportion falls to 11 per cent.), 36 per cent. of the Arcturian and 65 per cent. of the Capellan. The difference seemed to become more marked as the stars became fainter. It is curious, however, that for stars with very large proper motion —say over one second and a half per annum—the Arcturian type largely predominates. The Arcturians are perhaps the real "runaway stars." The steady-going members of the Solar cluster show no tendency to bolt the course. Such, at least, is the appearance which they present on a first inspection.

I remain, Sir, truly yours,
W. H. S. Monck.

THE CHEMICAL RESEARCHES OF JEAN SERVAIS STAS.

By Vaughan Cornish, B.Sc., F.C.S.

In the last month of last year the chemical world received with profound regret the news of M. Stas's death, at the advanced age of seventy-eight.

The name of Stas has been a household word among chemists for half a century, and his writings, the celebrated *Recherches sur les Lois des Proportions Chimiques*, have come to be regarded as among the canonical books of chemistry. In all that related to the experimental art Stas stood unsurpassed. The marvellous patience with which he matured his methods, and the skilful care with which the final experiments were carried out, stand recorded in his classical memoirs with that clearness and precision of expression characteristic of French scientific writings. Stas's work bore on one subject only, the determination of "atomic weights," with a view more particularly to ascertain if there existed any simple definite relation between the weights of the chemical atoms. In order to explain how this investigation came to be the mission of Stas's life, we must refer to the state of chemical theory in the second decade of the present century. At this time the laws of chemical combination had been formulated and accepted—the laws, viz., which may be epitomized by saying that "chemical elements combine together only in the proportion of their equivalent weights, or in simple multiples of those proportions." Dalton had propounded an explanation of these laws in his "Atomic Theory," according to which chemical combination was due to the union of chemically indivisible particles, the particle or atom of each element having its own particular fixed weight.

Dalton's theory, the next great generalization after Lavoisier's explanation of the phenomena of combustion, was the result of the discovery of definite and simple numerical relations between certain chemical quantities. It was natural that other minds, impressed by Dalton's theory, should seek for other such numerical relations in the hope of fresh discoveries of Nature's laws. In 1815 a paper appeared in Thomson's *Annals of Philosophy* by Dr. Prout, in which he pointed out certain apparent relations between the atomic weights of the elements as then determined. The idea was at once taken up by other chemists, and took shape in the following form, known as *Prout's Hypothesis*:—"The weight of the atom of each element is a simple multiple of the weight of the atom of hydrogen." The observed deviations were referred to errors of experiment, just as the apparent deviations from the laws of chemical combination were referred to experimental error.

It has been the life-work of Stas to investigate both assumptions, and to show that while the laws of chemical combination are rigidly exact the supposition of Prout is unsupported by experimental evidence.

Prout's hypothesis owes its importance in the history of science to the fact that it seemed to restore the old theory of the unity of matter, which appeared to have received its death-blow with the discovery of the chemical *elements*. But if the atom of each element be exactly once, twice or thrice the weight of the atom of hydrogen, then it is reasonable to suppose that the atoms of all elements contain only one kind of matter, and that the hydrogen atoms are the one class of ultimate particles of which all matter is built up. As the art of chemical analysis developed under the hands of the great Swedish chemist, Berzelius, it became evident that Prout's hypothesis was not tenable in its original form. It was revived, however, in a modified shape chiefly owing to the influence of Dumas. In the modified form, the hypothetical unit weight was that of the half-atom of hydrogen. Later on, Dumas was compelled to retreat yet further from the original position, and to take the quarter-atom of hydrogen as the greatest common divisor of the atomic weights. In this modified form the idea of Prout loses much of its interest, since the "quarter atom" of hydrogen is itself an unknown thing. Nevertheless, the idea of the oneness of matter always exerts a certain fascination, and to some minds this unity of matter appears to be almost a logical necessity. Hence the tenacity with which chemists have clung to the belief that apparent discrepancies were due to errors of experiment, rather than to the inaccuracy of Prout's hypothesis.

Stas began his researches on atomic weights with a strong prepossession in favour of the hypothesis. He chose for his determinations such substances as could be prepared in a high state of chemical purity, and worked with large quantities of substance in order to eliminate the effect of errors in weighing. A large number of experiments, which occupied several years, furnished him with extremely accurate values for the relative weights of the atoms of silver and the alkali metals, and of chlorine, bromine, and iodine. Moreover, the variety of methods employed served to eliminate possible systematic errors—errors, that is to say, not due to want of skill in the performing of an experiment, but due to the method itself. Each substance, moreover, was prepared in several different ways and from different natural sources. Not the least remarkable tribute to Stas's skill is the close accordance between the values he obtained for the atomic weights by different processes of determination. The numbers obtained in this first series of researches were closely accordant among themselves, and wholly at variance with those demanded by Prout's hypothesis. Stas concludes his memoir thus: "Prout's hypothesis must be looked upon as a pure delusion; the elements must be considered to be distinct entities, with no relation between their atomic weights."

The accuracy of Stas's work was admitted on all sides, but his conclusions were contested. The criticisms of the Genevese chemist, Marignac, are historically important, having led Stas to his second and more celebrated research. Marignac contended that it was far from being proved that the constituent elements of many chemical compounds were present exactly in the proportion of their atomic weights. It was possible that many chemical compounds contained normally a very small excess of one or other of their constituents. This criticism strikes at the basis of the Atomic Theory, since that theory is founded on the assumption that the laws of chemical

combination are mathematically exact. For half a century the scientific world had accepted the dictum that the laws of chemical combination were *lois mathématiques*, but the original experiments on which these laws were based were far from being models of accuracy. This fact was admitted by Stas, who undertook the laborious task of a re-examination of those laws, with a view to settle by the most exact methods whether these laws were in fact of mathematical exactness, or, like so many physical laws, only *lois limites* or approximate relations. In 1865, five years after the date of his first series of researches, appeared the *Nouvelles Recherches sur les Lois des Proportions Chimiques*. In this work Stas repeated the more important of his former determinations of atomic weights, with additional precautions. He also subjected to the most rigorous tests the laws of definite, constant, and equivalent proportions which had hitherto rested on the comparatively rough experiments of Dalton, Wollaston, and other workers of the early part of the present century. In this great work Stas confirmed, on the one hand, his previous conclusion that Prout's hypothesis was unsupported by experiment, but showed on the other that the laws of chemical combination, hitherto accepted on insufficient data, were, as far as experiment could prove, actual and veritable mathematical laws. It is impossible to over-estimate the benefit conferred upon science by a man who has the courage to devote years of patient labour to the re-examination of points such as this, and the re-investigation of supposed laws which have been accepted on the evidence of insufficient experimental data. Such work is much needed in the chemical world at the present time, when a vast superstructure of theory is being built upon a comparatively small number of approximate experiments with regard to the behaviour of substances in a state of solution.

From the point of view of the working practical chemist the most important aspect of Stas's researches is that relating to the preparation of chemical substances in a state of purity. Since Stas's time chemists have not been satisfied with the approximate purification of substances which in general sufficed the earlier experimenters. The approximate isolation or purification of substances is the first step in a chemical research; the complete purification is the most difficult and the most important part of exact research in the science. Stas's methods of purification have served as a model for all subsequent experimenters. In order to give a general idea of the character of his work we will describe a method he adopted for the purification of silver, a substance which is, as he says, the "pivot" of his determinations. Silver is a substance which, as Stas showed, can be obtained in a state of almost perfect purity. The way in which it resists oxidation, and the distinctive character and insolubility of certain of its salts, would lead one to suppose that its complete purification would be very readily effected. That this is not exactly the case will be evident from the following description of Stas's method. In order not to make the description unduly long, we omit the special methods of purifying the *reagents* used in the work. These reagents are water, nitric acid, hydrochloric acid, caustic potash, and milk-sugar. Each of these had to be submitted to special processes of purification, lest their use should introduce foreign substances into the silver.

Coinage silver was taken, and dissolved in very dilute nitric acid. Any gold present is left undissolved. The solution of the nitrate is evaporated to dryness, and heated till no more nitrous fumes are evolved. The salt is then dissolved in a small quantity of water. On filtering, any platinum present is left behind. The filtrate is then diluted with about thirty times its volume of water and an excess of hydrochloric acid added. All the silver is then precipitated or thrown down in the form of the insoluble chloride of silver. Any copper and iron present remain in solution. The liquid is poured off and the precipitate washed, first with dilute hydrochloric acid and then with water, till the washing appears to be pure water containing no trace of copper or of hydrochloric acid. This washing of a large quantity of a precipitate is a very lengthy and tedious operation, requiring days or weeks, according to the quantity of the precipitate. The washing is effected in this case by shaking up the precipitate with water in a stoppered flask, allowing the precipitate to settle, and pouring off the liquid. All the operations with chloride of silver were carried out in a room lighted by artificial light, since daylight, as is well known, effects a chemical change in the composition of chloride of silver. The chloride of silver, purified as above, is brought on to a cloth (previously washed with hydrochloric acid) and the water squeezed out. After drying, the silver chloride is pounded fine in a mortar, and *reduced* to the metallic state by warming for forty-eight hours with a solution of caustic potash and milk sugar (both carefully purified). The finely-divided metal is then fused, with special precautions to prevent access of impurities. By this process Stas hoped to obtain an ingot of perfectly pure silver, but found that, besides very slight traces of other substances, there remained an appreciable quantity, 2 parts in 100,000, of silica. Experience convinced Stas that no substance can be obtained absolutely pure except by distillation. He therefore subjected the silver obtained as above to the process of distillation from one cavity to another in a hollowed block of quicklime, made from white marble. The cavity having been previously heated by the oxy-hydrogen flame, in order to drive off any volatile substances such as soda, the silver was placed in the cavity and fused. No scum appeared on the surface, showing the absence of certain impurities such as iron, which under these circumstances would form a slag. The heat from the oxy-hydrogen flame was then increased till the metal began to boil. The vapour had at first a strong yellow tinge, showing that sodium was still present.

This, however, soon disappeared, the vapour of the silver showing no colour beyond a faint blue tinge. The absence of any green tint showed that the substance was free from copper. The metal having completely distilled into the second cavity, or receiver, in the lime block, it was found that absolutely no residue remained, the small quantity of silica, and any similar fixed substance of an acid character, having combined with the lime, and any oxidizable material having been burnt away by the flame of the oxy-hydrogen blow-pipe. By the above process Stas believed that he had obtained silver absolutely pure. Subsequently, however, Dumas showed that silver thus prepared absorbs, after distillation but while still molten, a certain quantity of oxygen which does not combine chemically with the silver but remains "occluded" in the metal. The elaborate precautions adopted by Stas were therefore not successful in obtaining even this well-known and characteristic substance in a state of perfect purity, though he subsequently determined the amount of oxygen present. But the practical chemist owes to Stas a proper appreciation of the difficulties attending the purification of substances, an appreciation of the necessity for taking every means to overcome these difficulties, and a knowledge of methods for the carrying out of this class of work; methods elaborated by Stas thirty years since, and which yet form the basis of many of the recent researches on the determination of atomic weights.

XUM

THE LIFE OF AN ANT.—II.

By E. A. Butler.

THE exact nature of the occupations to which a worker Ant devotes itself on attaining maturity, depends somewhat upon the species, but there are, as a rule, certain well-defined heads under which their labours may be classed. Besides the care of the young, which we have already noticed, their chief duties will consist of the construction, enlargement, and repair of the nest, the hunt for provisions, and the defence of the colony. The satisfying of these claims will usually be sufficient to occupy all their time, so that their life becomes one of incessant activity, and their industry is well known and proverbial. Their activity may extend even into the night, for if daylight does not suffice to complete necessary work, it must be continued after dark, and Sir John Lubbock mentions having watched an Ant which worked continuously for nearly sixteen hours, from early morning till late at night, carrying larvæ to the nest.

The character and situation of the nest depend entirely upon the species. The conical or rounded heap of earth or twigs, commonly called an "Ant hill," is not always present, nor does it follow that if Ant hills are made, each distinct hillock represents a separate and independent community. While it very frequently happens that an entire community is located under one roof, such is by no means universally the case, and, therefore, the terms "Ant hill" and "Ants' nest" are not necessarily interchangeable; often one colony will have three or four distinct dwellings, in any of which its members would be at home. Occasionally some communities build much more extensively than this. M. Forel, indeed, cites a case in which a single community of Ants of a very active species (*Formica exsecta*) possessed as many as two hundred dwellings, which were spread over a circular area measuring nearly a quarter of a mile in diameter. So completely had they appropriated this area that, with the exception of a few nests belonging to a very agile kind, no other species of Ant dared show their faces in it. Many and fierce, no doubt, had been the conflicts before this undisputed sway was established, for *Formica exsecta*, though a delicate insect, is a ferocious fighter. Sir John Lubbock thus graphically describes its battles: "They advance in serried masses, but in close quarters they bite right and left, dancing about to avoid being bitten themselves. When fighting with larger species, they spring on to their backs, and then seize them by the neck or by an antenna. They also have the instinct of acting together, three or four seizing an enemy at once, and then pulling different ways, so that she on her part cannot get at any one of her foes. One of them then jumps on her back and cuts, or rather saws, off her head."

While several Ant hills, as we have seen, may belong to one community, it sometimes happens, on the other hand, that a single hillock is shared by two communities, belonging to distinct species, one half being occupied by the one party and the remainder by the other, each maintaining a separate organization, though domiciled under a single roof. Thus the Yellow Ant (*Lasius flavus*), and one of the races of the Red Stinging Ant (*Myrmica scabrinodis*), not unfrequently live side by side in this way, and the distinctness of their establishments is evidenced by the fact that if the nest be disturbed, as for example by the dislodging of a stone, the two parties on rushing out to discover the cause of the disturbance will sometimes come into conflict, and a desperate struggle will take place, each apparently mistaking the other for the authors of the disaster. But there is another way in which a single Ant hill may shelter more than one species. Small and weak species sometimes live in the nests of larger and more powerful ones, completely intermixed with them, though the bond of connection is not always the same. There is a minute Ant called *Stenamma Westwoodii*, which is never found except in the nests of the Wood Ant and an allied species, and so dependent do they seem on their hosts, that when the latter change their abode, their little companions go with them. They are certainly not on terms of hostility to the larger species, which, indeed, take but little notice of them. Not so, however, with another minute Ant called *Solenopsis fugax*; these are real parasites; they make galleries in the walls of their hosts' nests, whence they issue to invade the nurseries of the latter and carry off the young as food. Once within their galleries, they are safe from retribution at the hands of their defrauded hosts, as the latter are too large to get into their tiny burrows. Sir John Lubbock has very aptly compared their depredations to what would be the state of affairs with us, "if we had small dwarfs, about eighteen inches to two feet long, harbouring in the walls of our houses and every now and then carrying off some of our children into their horrid dens."

But further, a smaller and weaker species may be in the nest of a larger one as slaves, or at any rate, helpers. Though the most remarkable of the slave-making species are not British, we have in this country one kind which is addicted to such habits. It is very much like the Wood Ant (*Formica rufa*), and is sometimes called the Red Ant, (*F. sanguinea*). (Fig. 4.) The worker has a red head and

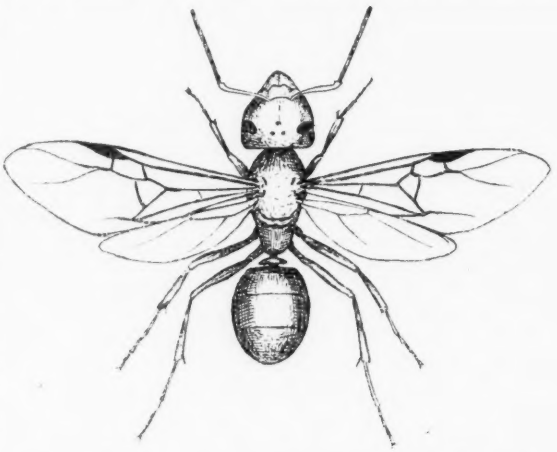

Fig. 4.—*Formica sanguinea*. Female. A slave-holding Ant. Magnified 4 diameters.

thorax, and a black body; it makes raids upon neighbouring nests of different species, capturing those larvæ and pupæ which will produce workers. These, on coming to maturity, finding themselves surrounded only by members of a different species, and having no young of their own to look after, calmly submit to their fate, adapt themselves to their circumstances and set to work to tend the young of their captors. In all Ants, the feeding of the young seems to be the first work to which newly-matured workers devote themselves, and it is certainly the most suitable employment for them until their skins are sufficiently hardened to make them fit to go out on foraging expeditions, when they may have to fight for their own lives or for those of their brethren. The practice of keeping slaves or assistants, of an inferior race, has not advanced in *F. sanguinea* to such an extent as it has in some foreign species, where

the captors are entirely dependent, even in the act of feeding, upon the exertions of the slaves. The Red Ant can apparently manage as well without slaves as with them; moreover, it does not seem to be particular about the species to which its helpers belong. Mr. F. Smith, of the British Museum, used to say that he had found the workers of four other kinds in the nests of this slave-owner.

The origin of slave-holding is obscure, but it seems likely that it may have arisen from the capture of pupæ of other species in time of scarcity, to be used as food. Ants in general are quite prepared to use the pupæ of other species for this purpose, and if on occasions of scarcity other nests were plundered and more pupæ carried off than were required for immediate consumption, the residue would probably mature in the nest of their captors, and might then begin to take on themselves the nursing function, to the manifest advantage of the other party, who might thus be induced to bring in more for the same purpose. Summarizing what has been said above, we have shown that a smaller and weaker species of Ant may be associated in the same nest with a larger and more powerful one in any of three ways—as a mere companion or lodger, or even perhaps pet, as a parasite, and as a slave or auxiliary.

The nature of the nest, and, in consequence, the employment of the workers in constructing it, varies with the species. Some, such as the Wood Ant and the *Formica exsecta* referred to above, construct great mounds composed of bits of stick, pine needles, bents of grass, fragments of fern-fronds, &c. These fragments are not in any way fastened together, but simply placed carefully one upon another, in such a way as to form a fairly compact mass, which is firm enough to resist a good deal of pressure. A large mound, of say two feet high, and ten or twelve feet in circumference, must contain many thousands of twigs, and when the number and the careful disposition of these is realised, one can form some notion of the vast amount of labour that its construction represents, all of which falls to the share of the workers. For the collection of the materials, the ground for large distances round the nest is traversed again and again, the army of collectors following certain definite tracks, which, by the trampling of innumerable little feet, in time become distinctly marked roads, and from which, in order to improve them, any obstacles that are not too large nor too firmly fixed are intentionally removed. A suitable fragment having been discovered by any member of the collecting party, the little creature seizes its prize with its jaws in such a way as least to incommode its own movements on its return journey, keeping it, if possible, pointing forward and raised well off the ground. On arriving at the nest it places its load in position, and in doing so a good deal of judgment and discretion must be exercised, for the pile is not simply to be an indiscriminate heap, but is to be traversed through and through with tunnels and passages, large enough to allow free passage to and fro for the inhabitants, and regular enough to allow of pupæ being laid out along them; and openings must be left at different spots on the sides of the mound to serve as entrances to the nest. These also must be so arranged that they can be closed at night by putting up barricades of interlacing fragments, a duty which devolves on sentinel workers. It is nothing short of marvellous that so many hundreds and even thousands of individuals should be able to act in concert, and contribute piece by piece to the raising of so deftly and securely constructed an edifice, without any single presiding genius to lay the plans and see to their execution, and it seems to point to a remarkable degree of intelligence and power of adaptation on the part of the workers, such as to enable them to fit in and utilize to the best advantage fragments of all sorts, sizes, and shapes. It is also a remarkable fact that the symmetry of the nest is always well preserved; its outline is regular, and the slope even on all sides, although the builders, in consequence of their small size as compared with the nest, and their position on its surface when in the act of building, cannot possibly see more than a very small portion of its outline as they deposit and arrange their load.

The above-ground structure just described is not the whole of the nest; there will be tunnels below ground as well, underneath the dome, and the earth from these has to be removed, fragment by fragment, by the jaws of the workers, and carried up above. The growth of the nest, in both its divisions, must, of course, keep pace with the increase of population, which involves ceaseless labours on the part of the architects and builders; while damage to the structure, caused by such accidents as the fall of objects upon it, will call forth the energy and skill of the engineers of the community to effect a speedy repair of the breach. We have thus seen that, in the case of such a species as the Wood Ant, the collection of materials for the construction of the nest involves long and frequent journeys, and no inconsiderable proportion of the time of the labourers must, therefore, be spent in this employment. As its popular name implies, this Ant usually forms its nest in woods, preferring pine plantations, for here there will be abundant material strewed about the ground wherewith to form its conical piles. It is found all over the country. The slave-making Ant, on the other hand, though in appearance very similar, prefers to excavate its galleries in banks, or round the stumps of gorse, and is found chiefly in sandy localities in the southern counties, especially Surrey and Hampshire. *Formica exsecta*, the species referred to above as having such extensive colonies, forms mounds in which bits of fern-fronds, ling, and grass are used. *F. congerens*, an insect very much like the Wood Ant, also makes mounds, and the Rev. Farren White mentions a large one he found on a sloping bank covered with fern, heather, and gorse, on the margins of a stream; it was about a foot high and over eighteen feet in circumference, and had seven entrances.

Our other Ants do not form mounds of this sort, but chiefly excavate the soil, or the trunk of a tree. Some, like *F. cunicularia*, mine under stones, taking advantage of the protection thus afforded. The common *F. fusca*, one of the chief slaves of the Red Ant, delights in banks, especially such as face the south. The margins of the footpaths in our gardens often bear witness to the presence of the Garden Ant (*Lasius niger*), in the little heaps of grains of earth which the workers have laboriously brought out one by one from the soil beneath; this species will, however, sometimes excavate the stumps of trees. The Black Ant (*L. fuliginosus*) usually fixes on the bottom of an old post or the decaying stump of a tree for its abode; it excavates the wood in galleries, the walls of which always become stained black in consequence of its presence. The abodes of the common Yellow Ant (*L. flavus*) are the rounded grassy mounds, generally something under a foot in height, which are so familiar in meadows and heathy places. In this instance the dome is composed of earth, and though this, as well as the ground beneath, is excavated through and through with tunnels, the growth of the turfy grass is not interfered with, and hence great firmness and compactness is imparted to the nest. As the colony multiplies, the dome increases in size by the transference to the outside of the particles obtained from the excavations of the interior, in such a way as to distribute it uniformly and not destroy the symmetry of shape. As the

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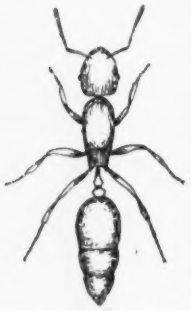


Fig. 5.—Queen of *Leptothorax acervorum*, deprived of wings. A burrower under bark. Magnified 8 diameters.

deposits are gradual, grain by grain, they do not prevent the continued growth of the grass, which is ever extending itself outward, forming an excellent thatch and acting as an efficient watershed in case of rain. The insect can, however, adapt itself to circumstances, and in hilly country where grassy mounds are impossible will avail itself of the protection afforded by loose fragments of rock. The Red Stinging Ants (*Myrmica*) similarly excavate the ground under stones, while a neat little reddish species called *Leptothorax acervorum* (Fig. 5) forms galleries under the bark of trees, especially fir stumps. Lastly may be mentioned a thin and wiry small black species, the workers of which have a squarish head which is streaked with longitudinal furrows; it is called *Tetramorium cæspitum* (Fig. 6), and is found at various places along our south-eastern coasts, seeming for some reason or other to prefer to be near the sea. It also shams death when disturbed, folding up its legs and lying quite still. We have thus seen that the lives of the worker Ants in all these latter species are spared much of the labour which falls to the lot of the mound-builders, and whatever journeys are undertaken will be, not for the accumulation of building material, but only for obtaining food.

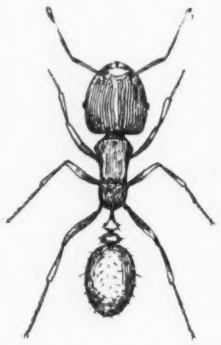


Fig. 6.—*Tetramorium cæspitum*. Worker. A sea-coast Ant. Magnified 10 diameters.

Ants feed chiefly upon insects of various kinds, honey, honey-dew, and fruit, and for a large nest the daily bill of fare must be extremely varied and comprehensive. In nests of only moderate size, the number of individuals will run up to thousands, while of very large ones Sir John Lubbock's opinion is that "perhaps London and Pekin are about the only human cities which can compare with them" as to population. The amount of food that such multitudes would daily need is difficult to conceive, but so far at least as animal food is concerned, the requirements of a large nest would probably demand that many thousands of small insects of different kinds should be slaughtered every day. The hunting and gathering in of this quarry is largely left to individual enterprise, at least amongst the species found in this country, no organized foraging parties being usually employed; possibly, however, certain special individuals may be sometimes told off for the work. The corpse of a larger insect, such as a good-sized beetle or a bumble bee, is an excellent find; if too large and heavy to be dragged to the nest in its entirety, even by the united efforts of a gang of labourers summoned by communications from the original discoverer of the booty, the carcase must be cut up where it lies and carried home piecemeal, an operation not difficult of accomplishment by insects that always carry in their mouths a good pair of scissors in the form of mandibles. The collection of honey-dew leads many Ants to climb trees and plants, and wander over their leaves in search of the deposit, or of the aphides which produce it.

THE FACE OF THE SKY FOR APRIL.

By Herbert Sadler, F.R.A.S.

LARGE groups of spots and faculæ continue to diversify the solar surface. There will be a total eclipse of the Sun on the evening of the 26th, but as it will not be visible in the northern hemisphere, it need not be further noticed here. The following are conveniently observable times of some Algol-type variables (*cf.* "Face of the Sky" for March). S Cancri.—April 3rd, 11h. 1m. P.M.; April 22nd, 10h. 17m. P.M. δ Libræ.—April 6th, 10h. 12m. P.M.; April 13th, 9h. 46m. P.M.; April 20th, 9h. 10m. P.M.; April 27th, 8h. 54m. P.M. U Coronæ.—April 5th, 11h. 28m. P.M.; April 12th, 9h. 10m. p.m.

Mercury is an evening star during the first of the month, and is well placed for observation during the beginning of April, in the sense of setting some considerable time after the Sun. His brightness, however, has notably diminished since the third week in March. On the 1st he sets at 8h. 30m. P.M., just two hours after the Sun, with a northern declination of 14° 24′, and an apparent diameter of $7\frac{3}{4}''$, $\frac{4}{10}$ths of the disc being illuminated. On the 5th he sets at 8h. 31m. P.M., or 1h. 52m. after sunset, with a northern declination of 15° 39′, and an apparent diameter of $8\frac{1}{2}''$, $\frac{23}{100}$ths of the disc being illuminated. On the 10th he sets at 8h. 16m. P.M., or about $1\frac{1}{2}$ hour after the Sun, with a northern declination of 15° 55′, and an apparent diameter of 10·0″, $\frac{1}{10}$th of the disc being illuminated. After this he rapidly approaches the Sun, being in inferior conjunction with him at 4 P.M. on the 19th. He describes, while visible, a very short arc in Aries, without approaching any naked eye star.

Venus is now a resplendent object in the evening sky, being visible to the naked eye before sunset towards the end of the month. On the 1st she sets at 10h. 51m. P.M., 4h. 20m. after sunset, with a northern declination of 21° 30″, and an apparent diameter of $17\frac{3}{4}''$, $\frac{64}{100}$ths of the disc being illuminated. On the 15th she sets at 11h. 26m. P.M., $4\frac{1}{2}$h. after sunset, with a northern declination of 24° 53″, and an apparent diameter of 20·0″. About $\frac{58}{100}$ths of the disc are then illuminated, the brightness of the planet being about $\frac{2}{3}$rds of what it will be at the beginning of June. On the 30th she sets at 11h. 49m. P.M. (being at her greatest eastern elongation [$45\frac{1}{2}°$] at 5h. A.M. that morning), with a northern declination of 26° 46′, and an apparent diameter of $22\frac{3}{4}''$, just one-half of the disc being illuminated. At about 8h. 30m. P.M. on the 27th the planet will be $\frac{3}{4}'$ north of a $9\frac{1}{4}$ magnitude star. During the month she describes a direct path through nearly the whole of Taurus, being closely south of the Pleiades on the evenings of the 2nd, 3rd, and 4th.

Both Mars and Jupiter are, for the observer's purposes, invisible.

Saturn is an evening star, and is very well situated for observation. He rises on the 1st at 4h. 41m. P.M., with a northern declination of 4° 3′, and an apparent equatorial diameter of 19·2″ (the major axis of the ring system being 44·1″ diameter and the minor 1·0″). On the 30th he rises at 2h. 37m. P.M., with a northern declination of 4° 42′, and an apparent equatorial diameter of 18·6″ (the major axis of the ring system being 42·8″ in diameter and the minor 0·4″). The following phenomena of the satellites may be observed (the times are given to the nearest quarter of an hour). April 7th, $0\frac{1}{4}$h. A.M., Dione, eclipse reappearance. April 9th, $3\frac{1}{2}$h. A.M., Rhea, eclipse reappearance. April 12th, Tethys, eclipse reappearance; 9h. P.M., shadow of Titan in central transit. April 14th, $1\frac{1}{2}$h. A.M., Tethys, eclipse reappearance. April 15th, $10\frac{3}{4}$h. P.M., Tethys, eclipse re-

appearance. April 17th, 8h. P.M., Tethys, eclipse reappearance; 11h. P.M., Dione, eclipse reappearance. April 19th, 4h. P.M., Iapetus, at greatest eastern elongation. April 28th, 8½h. P.M., possible transit of Titan's shadow just skirting limb of planet; 10h. P.M., Dione, eclipse reappearance. On the 6th, at about 0½h. A.M., a 9¼ magnitude star may suffer occultation by the planet's southern limb. During April Saturn describes a retrograde path through a portion of Virgo, barren in naked-eye stars.

Uranus is well situated for observation, coming into opposition with the Sun on the 23rd, at a distance from the earth of about 1,627,650,000 miles. He rises on the 1st at 8h. 30m. P.M., with a southern declination of 12° 42′, and an apparent diameter of 3·8″, the apparent star magnitude of the planet being 5·4 in the photometric scale. On the 30th he rises at 6h. 28m. P.M., with a southern declination of 12° 17′, and an apparent diameter of 3·8″. During the month he describes a retrograde path through a barren region of the sky to the W.N.W. of λ Virginis. A map of the path of Uranus is given in the *English Mechanic* for February 12th. Neptune is an evening star, but should be looked for as soon after dark as possible, as he is rapidly approaching the west. On the 1st he sets at 11h. 32m. P.M., with a northern declination of 19° 56′, and an apparent diameter of 2·5″. On the 30th he sets at 9h. 43m. P.M., with a northern declination of 20° 6′. During April he describes a short direct path from a little to the N.W. to a little to the N.E. of ε Tauri.

Shooting stars are fairly plentiful in April, the best marked shower being that of the Lyrids, with a radiant point in R.A. 18h. 0m. +33°. The radiant point rises on the evenings of the 19th and 20th, when the maximum occurs at about 6h. 27m. P.M., and souths at 4h. 8m. A.M.

The Moon enters her first quarter at 6h. 21m. A.M. on the 4th; is full at 6h. 26m. A.M. on the 12th; enters her last quarter at 6h. 0m. A.M. on the 20th; and is new at 9h. 46½m. P.M. on the 26th. She is in apogee at 11·4h. P.M. on the 11th (distance from the earth 252,580 miles); and is in perigee at 9·2h. A.M. on the 26th (distance from the earth 222,090 miles). Her greatest western libration occurs at 6h. 58m. A.M. on the 4th, and her greatest eastern at 10h. 19m. A.M. on the 22nd.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

Solution of Problem in March number.—1. Q to K2, anything. 2. B to R6, and mates next move.

Correct Solution received from Alpha.

Alpha.—Your solution of the four-move problem by 1. Kt to Kt7 goes extremely near. There is, however, one objection. After 1. . . . P to QB4, 2. Kt to B6ch, K to K5; 3. Q to Kt5, P to KB4 saves the mate. 1. B to B3 (threatening B × P, &c.) is the correct key-move.

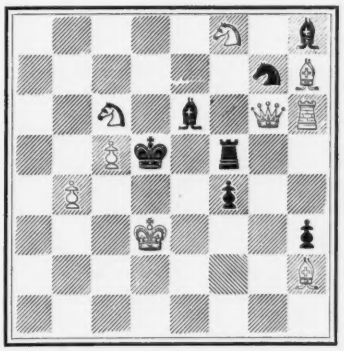
W. T. Hurley.—We hope to adopt your suggestion on future occasions. The winner, like yourself, was a novice in solution tournaments.

N. Pennefather.—At present there is no solution tourney in progress. Should there be another this year, it will be announced in this column. In the March problem, after 1. K × P, P to Kt5; White cannot mate in two more moves.

PROBLEM.

By W. E. B.

Black.



White.

White to play, and mate in two moves.

CHESS INTELLIGENCE.

The match for the championship at Havana resulted in favour of Mr. Steinitz by ten games to eight, five games only being drawn. As soon as the winner began to have recourse to the Close Game, the result was no longer in doubt, for Mr. Tschigorin, as was shown in the previous match, has no chance in this opening.

The following were the results of the various openings in the match:—Evans Gambit, played 8 times by Tschigorin, who won 4, lost 1 and drew 3. Ruy Lopez, played 4 times by Steinitz and once by Tschigorin, Steinitz winning 4 and drawing the other. Two Knights defence, opened 4 times by Steinitz, who won 1 and lost 3. Close Game, played 3 times by Steinitz, who won all 3. In the other three games (Scotch, and Steinitz Gambits), all opened by Tschigorin, an even score was made. If Tschigorin had confined himself to the Evans Gambit, his strongest opening, and Steinitz had relied solely on the Ruy Lopez and Close Game, the score in these openings would have been Steinitz 8, Tschigorin 4, drawn 4; a result which is probably in accordance with the actual relative strength of the two players. The closeness of the match must, therefore, be ascribed to unwillingness on the part of Mr. Steinitz to abandon his unsound variation of the Two Knights Defence.

The result of the Handicap Tournament of the British Chess Club is announced as follows:—1. Mr. C. D. Locock (won 6, drawn 3, lost 0, unplayed 3). 2. Mr. J. L. Cope (won 5, drawn 1, lost 2, unplayed 4). 3. Mr. H. W. Trenchard (won 8, drawn 0, lost 4, unplayed 0); bracketed equal with Mr. Handford (won 4, drawn 2, lost 3, unplayed 3). It will be noticed that Mr. Trenchard was more successful than the other competitors in inducing his opponents to play. But for this, he would probably have come out at least a place higher. The committee could hardly do otherwise than score unplayed games in favour of those competitors who had shown their willingness to play.

The championship of the City of London Chess Club has been won by Mr. Moriau, who, in the final tie, won a brilliant game from Dr. Smith.

The National Masters' Tournament of the British Chess Association began at the British Chess Club on March 7th, and was brought to a successful conclusion on the 17th.

XUM

There were twelve competitors, who played one game each a day, the evening being reserved for unfinished games. The final score was as follows :—

	Won.	Drawn.	Lost.	Total.
E. Lasker	8	2	1	9
Jas. Mason	6	3	2	7½
R. Loman	6	2	3	7
H. E. Bird	6	1	4	6½
C. D. Locock	4	5	2	6½
R. F. Fenton	3	5	3	5½
F. J. Lee	4	3	4	5½
N. Jasnogrodsky	5	0	6	5
L. Van Vliet	3	3	5	4½
J. Mortimer	2	3	6	3½
A. Rumboll...	3	0	8	3
G. H. D. Gossip	1	3	7	2½

The first prize accordingly went to Mr. Lasker, the Berlin champion, and certainly one of the finest players in Europe, perhaps even the greatest Chess genius since Paul Morphy ; Mr. Mason, who never quite wins a tournament, owing to his drawing tendencies, took the second prize ; and Mr. Loman, the champion of Holland, and last year of the City of London Club and the Divan, the third prize. Messrs. Bird and Locock divide the fourth prize. Mr. Fenton was a little fortunate in some of his drawn games, Mr. Lee being unfortunate in losing to both Messrs. Rumboll and Gossip. Mr. Jasnogrodsky started badly, but afterwards played some fine games. Mr. Van Vliet, except for his victory over Mr. Bird, hardly played up to his reputation ; his end-game play was especially careless. Mr. Mortimer, as usual, proved himself formidable to the strongest players, but threw away at least one certain victory.

A match played at Simpson's Divan, between Mr. H. E. Bird and Mr. R. Loman, resulted in a victory for the former by four games to two. The match was practically for the Divan Championship, Mr. Bird having won the last level tournament there, while the previous tournament was won by Mr. Loman.

The result of the National Masters' Tournament, in which Mr. Loman won his game with Mr. Bird, confirms the impression that the two players are, in their different styles, as evenly matched as they could be.

The Amateur Championship of the British Chess Association has been won by Mr. Jones-Bateman. The position of the other competitors is not yet decided.

The following was the fourth game in the recent Championship Match at Havana :—

[Ruy Lopez.]

White (Steinitz).	Black (Tschigorin).
1. P to K4	1. P to K4
2. Kt to KB3	2. Kt to QB3
3. B to Kt5	3. Kt to B3
4. P to Q3	4. P to Q3
5. P to B3	5. P to KKt3
6. QKt to Q2	6. B to Kt2
7. Kt to Bsq	7. Castles
8. B to R4 (*a*)	8. Kt to Q2 (*b*)
9. Kt to K3	9. Kt to B4
10. B to B2	10. Kt to K3
11. P to KR4 (*c*)	11. Kt to K2
12. P to R5	12. P to Q4 (*d*)
13. RP × P	13. BP × P
14. P × P	14. Kt × P
15. Kt × Kt	15. Q × Kt
16. B to Kt3	16. Q to B3 (*e*)
17. Q to K2	17. B to Q2
18. B to K3	18. K to Rsq
19. Castles QR	19. QR to Ksq
20. Q to Bsq (*f*)	20. P to QR4
21. P to Q4	21. P × P
22. Kt × P	22. B × Kt (*g*)
23. R × B	23. Kt × R (*h*)
24. R × Pch	24. K × R
25. Q to Rsqch	25. K to Kt2
26. B to R6ch !	26. K to B3
27. Q to R4ch	27. K to K4
28. Q × Ktch	28. K to B4
29. P to Kt4 (*i*)	Mate.

Notes.

(*a*) The opening is of the kind made in Germany. In the second game of the present match Mr. Steinitz played Kt to K3 at this point, whereupon Black replied, 8 . . P to Q4 ; 9. Q to B2 ; P × P ; 10. P × P ; Kt to Q2, and got a good game.

(*b*) This and the succeeding manœuvres with the Knight take up much valuable time. He might play P to Q4 while he can.

(*c*) Vigorously taking advantage of Black's unusual passiveness.

(*d*) This makes matters worse. He might try R to Ksq and Kt to Bsq, as suggested by Mr. Lee. On his next move he would do better by taking with the RP.

(*e*) In order to prevent Kt to Kt5 by keeping some pressure on White's KKtP. White now proceeds to finish his development before the final attack.

(*f*) A fine retreat, with a view to the sacrifice of the Rook if he gets a chance.

(*g*) If 22 . . Kt × Kt ; 23. R × Pch, K × R ; 24. Q to Rsq ch, &c. The capture made is, however, a mistake. He might play 22 . . Q to R3.

(*h*) A blunder which gives Mr. Steinitz his pretty finish. R to K2 was surely better.

(*i*) The whole finish is most artistic, and the game a fine specimen of Mr. Steinitz's style, when he likes to be brilliant.

Correction.—There were one or two misprints in the review of Mr. Freeborough's *Chess Endings* last month. In No. 40, for 6. K to R4 *read* 6. K to R5 ; and for 7. K to R5 *read* 7. K to R4. In No. 130, for 4, 5, 6, *read* 6, 7, 8. Mr. Freeborough has called our attention to these errors.

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